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ABSTRACT

In 1976 Congress terminated the largest federal program of direct student assistance, the GI Bill. This study followed in the wake of growing dissatisfaction with the existing program and of recruiting difficulties experienced by the services during the late 1970s. This report presents the history and results of the Educational Assistance Test Program, a large-scale experiment conducted by the Department of Defense during fiscal year 1981. The experiment was mandated by Congress to determine the effectiveness of expanded postservice educational assistance programs for attracting more high aptitude high school graduates into military service. The results suggest that a narrowly targeted program can increase enlistments by an amount that is out of proportion to the limited number of jobs covered, and do so at a cost per additional recruit that probably is lower than that for a general program under which everyone brought in by the program may ultimately receive the benefit. Implications for future policy are discussed which provide a framework for the design of a relatively cost-effective program of military educational benefits. (PN)

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Enlistment Effects and Policy Implications of the Educational Assistance Test Program

Richard L. Fernandez

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Enlistment Effects and Policy Implications of the Educational Assistance Test Program

Richard L. Fernandez

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PREFACE

This report presents the history and results of the Educational Assistance Test Program, a large-scale experiment conducted by the Department of Defense during fiscal year 1981. The experiment was mandated by Congress to determine the effectiveness of expanded postservice educational assistance programs for attracting more high-aptitude high school graduates into military service. It followed in the wake of growing dissatisfaction with the existing program and of recruiting difficulties experienced by the services during the late 1970s.

This study was conducted as part of Rand's Manpower, Mobilization, and Readiness Program, sponsored by the Office of the Assistant Secretary of Defense (Manpower, Reserve Affairs, and Logistics), under contract number MDA 903-80-C-0652. A companion Rand report analyzes a survey conducted during the experiment: Bruce R. Orvis and Jennifer A. Hawes, *1981 Survey of Military Applicants* (forthcoming). A summary volume gives the main features of the test and the principal study findings: J. Michael Polich, Richard L. Fernandez, and Bruce R. Orvis, *Enlistment Effects of Military Educational Benefits*, N-1783-MRAL, February 1982. Rand's analysis of an earlier experiment appeared in Gus W. Haggstrom et al., *The Multiple Option Recruiting Experiment*, R-2671-MRAL, November 1981.

SUMMARY

In 1976 Congress terminated the largest federal program of direct student assistance, the GI Bill. Although past servicemembers would continue to be eligible for GI Bill benefits, persons enlisting after December 31, 1976, were offered only the less generous Veterans Educational Assistance Program (VEAP). In switching from the GI Bill to VEAP, Congress substantially reduced the maximum dollar amount of the services' educational benefit offering. To receive benefits under VEAP, enlistees must make monthly contributions of $25 to $100, which are later matched two-for-one by the Veterans Administration.[1] The maximum individual contribution is $2,700, yielding a total possible payout of $8,100 to meet expenses for college or vocational training.

Recruiting results in the years following the introduction of VEAP were disappointing. Although no direct link has been established between the termination of the GI Bill and these poor results, in 1978 Congress moved to enhance the basic VEAP benefit, calling for a test of lump-sum enhancements, called "kickers," for high-quality youths —high school graduates scoring at or above the 50th percentile on the military aptitude test. The results of this early test were inconclusive, however, and the recruiting problems continued. Dissatisfaction with VEAP mounted in the services and in Congress, where proposals to expand or replace VEAP were actively considered in 1980.

The result was a large-scale test of more generous educational benefit programs, mandated by Congress for fiscal year 1981. Three parties were involved in the development of the test programs: (1) the Army requested permission to test larger kickers; (2) the House of Representatives authorized a test of a new program modeled after the World War II GI Bill; and (3) the Senate Armed Services Committee called for a test of VEAP without the requirement that the individual contribute. Each program was offered in a "test cell" consisting of a set of geographically dispersed areas of the country. In the remainder of the country—the control cell—the services' educational benefit offerings remained as they were during 1980. The Rand Corporation designed a set of area assignments that matched the cells on a number of criteria to ensure that, at the beginning of the test, recruiting

[1]Responsibility for providing the matching funds has recently been transferred to the Department of Defense.

conditions were approximately the same in all the cells. The test was set to run from December 1, 1980, through September 30, 1981.

The details of the four test programs were as follows:

- *Control program:* basic, contributory VEAP in all services; kickers of up to $6,000 for qualifying Army enlistees only.
- *Ultra-VEAP kicker program:* identical to control program, except Army kickers raised to a maximum of $12,000.
- *Noncontributory VEAP program:* DoD payment of the VEAP contribution for qualifying enlistees in all services; Army kickers of up to $6,000.
- *Tuition/Stipend program:* for qualifying enlistees in all services; tuition assistance ($1,200/year) plus subsistence allowance ($300/month), for up to four academic years; benefits indexed for inflation; option for benefits to be transferred to dependents or cashed out upon reenlistment; no extra benefits for Army enlistees.

Three features of the test programs were particularly important. First, as noted above, in the control cell each of the services offered only the educational benefit plan it had offered nationwide in 1980. Thus the control cell indicated what the year-to-year change in recruiting conditions would have been nationwide had there been no special test programs. Second, all of the test programs, and the Army kickers in the control program, were limited to certain enlistees: high-quality individuals enlisting in one of a designated set of "critical skills." The designated specialties covered a majority of Army job openings, but only about 20 percent of Air Force and 10 percent of Navy openings.[2] Third, in all of the test cells but one—the Tuition/Stipend cell—the Army offered substantially greater benefits than the Navy, Air Force, and Marine Corps.

To determine the gross enlistment effects of the programs in the three services, we compared each test cell with the control cell in terms of their gains in high-quality male enlistments between a base period (December 1979-September 1980) and the test period (December 1980-September 1981). For example, in the Army the control cell showed a 21.7 percent gain, and the Ultra-VEAP kicker cell a 32.3 percent gain, yielding an estimate of the effect of the kicker program of approximately 9 percent (1.323 divided by 1.217). We also developed a regression model to control for outside factors affecting enlistments—local labor market conditions and recruiting effort—but the regression results did not substantially change our estimates.

[2]The test in the Marine Corps was conducted in a different manner than in the other services, and was not examined in this study.

Each of the test programs increased enlistments of high-quality males in at least one of the services. The Army's Ultra-VEAP kicker program raised Army enlistments by 9 percent. The Noncontributory VEAP program, offered in all the services, raised enlistments by a statistically significant amount only for the Air Force (5 percent). Finally, the Tuition/Stipend program showed increases of 10 and 8 percent in the Navy and Air Force, respectively, but reduced Army enlistments by 6 percent. This decline was apparently a result of the equalizing of benefits across the services under that program. The converse effect, however, did not occur: Navy and Air Force recruiting was not hurt by the Army's kickers in the Ultra-VEAP cell. These cross-service effects indicate that educational benefits may be less effective at bringing recruits into the Army than into the Navy and Air Force, and that the Army could be hurt if a new program of educational benefits did not maintain the current differential for the Army.

An examination of enlistments by military specialty indicates that the test programs were able to channel enlistees into the eligible specialties in the Army and Air Force (specialty data were not available for the Navy). The skill channelling effect was pronounced in the Army, where enlistments in the eligible specialties under the Ultra-VEAP kicker program rose sufficiently to absorb all of the total enlistment gain attributable to the program. Even the Tuition/Stipend program, which produced no enlistment gain for the Army, shifted recruits into the covered skills. The data also showed that when the Army list of eligible specialties was expanded beyond the combat arms, recruiting for the combat arms was hurt. Skill channelling was much less apparent in the Air Force: many enlistees attracted to the Air Force by the Noncontributory VEAP and Tuition/Stipend programs chose specialties that did not qualify them for the special benefits. The results suggest that a narrowly targeted program can increase enlistments by an amount that is out of proportion to the limited number of jobs covered, and do so at a cost per additional recruit that probably is lower than that of a general program under which everyone brought in by the program may ultimately receive the benefit.

The test results have the following implications for future policy:

- Serious consideration should be given to retaining a contribution requirement in any new military educational benefits program. The requirement does not discourage enlistments, but it does reduce costs.
- Skill targeting should be used as an additional means for limiting program costs. A targeted program can channel benefit-attracted enlistees into hard-to-fill specialties, but more

important from a cost standpoint, it ensures that most of the program dollars spent go to those enlistees with the greatest commitment to pursuing further education.

- In the design of a new program, the special problems faced by the Army should be recognized. The test has shown that one means of assisting the Army is to build more generous educational benefits into the Army program. However, some alternative enlistment incentive or program might be a more cost-effective way of giving the Army any special help it might need.

These conclusions provide a framework for the design of a relatively cost-effective program of military educational benefits. They should not be taken to imply, however, that a new program, or even retention of the existing VEAP (plus Army kickers), is necessarily desirable. Although the Educational Assistance Test Program has shown that a contributory, targeted program would be more cost effective than a general entitlement, it is not currently possible to determine exactly how costly such a program would be. It is important, therefore, to make the policy decision only after a close examination of the effectiveness and costs of alternative means—bonuses, additional recruiters, or advertising, for example—for bringing highly qualified individuals into the military services.

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I. INTRODUCTION

For almost 40 years military veterans have received federal educational assistance under three GI Bills. By September 1979, nearly 18 million people had used GI Bill benefits—including 57 percent of the Vietnam era veteran population—at a total cost to the government of more than $49 billion.[1] These expenditures have been viewed as serving a number of purposes, among them

> (1) enhancing and making more attractive service in the Armed Forces of the United States, (2) extending the benefits of a higher education to qualified and deserving young persons who might not otherwise be able to afford such an education, (3) providing vocational readjustment and restoring lost educational opportunities to those service men and women whose careers have been interrupted by reason of active duty . . . and (4) aiding such persons in attaining the vocational and educational status which they might normally have aspired to and obtained had they not served their country.[2]

With the end of conscription in 1973, most of these purposes seemed less relevant. In that year, the Interagency Task Force on the GI Bill and the All-Volunteer Force argued that veterans' benefits for new servicemembers "should be continued only to the extent Defense needs them to meet its manpower goals . . .," and called for the elimination of GI Bill educational benefits.[3] In the ensuing debate, the question of cost became paramount. From $252 million in fiscal year 1967, annual GI Bill expenditures had grown to $5 billion in fiscal year 1976, more than half of all federal direct student assistance.[4] Acting to reduce those costs, Congress passed Public Law 94-502, the Veterans' Education and Employment Assistance Act of 1976. The Act eliminated GI Bill eligibility for individuals entering the military services after December 31, 1976, and established the new Veterans Educational Assistance Program (VEAP). The new program's benefits were less than half as generous as those under the GI Bill, and required the individual to contribute to the plan while in the service in order to receive benefits later.

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[1]Veterans Administration (1980), pp. 17-19. "Vietnam era veterans" officially refers to those who served between August 5, 1964, and December 31, 1976.

[2]38 U.S. Code 1851.

[3]Quoted in Office of Assistant Secretary of Defense (Manpower, Reserve Affairs, and Logistics)—OASD(MRA&L)—(1980b).

[4]OASD(MRA&L) (1980b), p. 2-2.

The introduction of VEAP marked an important change in thinking on the proper role of veterans' educational assistance. Notably, the stated goals of the new program did not include providing readjustment assistance or restoring lost educational opportunities. The Senate Armed Services Committee did not reject these purposes completely: "The Committee believes that most of those purposes are applicable to those entering the service today and believes that the benefits, which will be provided by chapter 32 [VEAP], will facilitate the transition from military to civilian life that each service person must ultimately make."[5] Nonetheless, it clearly saw VEAP as primarily an enlistment incentive: "to promote and assist the all volunteer military program of the United States by attracting qualified men and women to serve in the Armed Forces."[6] More recent discussions in Congress have also emphasized this role for veterans' educational assistance.[7] Until very recently, however, there was no mechanism to assess the effectiveness of educational benefits in meeting this objective.

During 1981, a nationwide experiment—the Educational Assistance Test Program—was conducted to evaluate the effects of educational benefits on enlistments. The experiment was mandated by Congress to test several alternative programs that enhanced or replaced VEAP with more generous benefits for high-quality enlistees.[8] The principal questions addressed by the test were:

- How effective were each of the test programs for attracting high-quality recruits?
- What would be the effect on recruiting of eliminating the VEAP contribution requirement?
- Can a targeted program—one restricted to enlistees entering certain hard-to-fill specialties—increase total enlistments, and what is the effect of targeting on the specialty distribution of enlistees?
- Can an educational benefit program be designed to give the greatest help to the Army—the service with the most acute recruiting problems—without adversely affecting recruiting for the other services?

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[5]U.S. Senate (1976), p. 61.

[6]38 USC 1601. The author is not alone in thus interpreting the intent of of Congress; see OASD(MRA&L) (1980b), p. 2-3.

[7]See, for example, testimony on H.R. 1400—The Veterans' Educational Assistance Act of 1981, in U.S. House of Representatives (1981), Vol. I. These discussions also addressed the important problem of the adverse effects on *retention* that might be caused by a generous program of postservice educational benefits.

[8]An earlier test of more modest enhancements, conducted in 1979, yielded inconclusive results; see Haggstrom et al. (1981).

- How can a new program of educational assistance best be structured to assist recruiting while minimizing costs?

The purpose of this report is threefold. Its primary purpose is to report on a study of the 1981 Educational Assistance Test Program that was designed to answer the above questions.[9] To place the test results in context, however, it first reviews the history of military educational assistance programs during the period of the All-Volunteer Force, and describes the considerations that led to the test. Finally, it draws conclusions and makes recommendations to assist in the formulation of future policy on the structure of military educational assistance programs.

Section II of this report provides background—the switch from the GI Bill to VEAP, the provisions of the new program, and the results of previous research. Section III describes the origins, provisions, design, and limitations of the 1981 experiment, and poses the question: What can the test show? Section IV presents the data gathered for this study and the methodology used in the analysis. Sections V and VI present the test results, Sec. V for total enlistments and Sec. VI on the skill channelling effects of the test programs. Section VII summarizes the major study findings, and offers several policy recommendations. Appendix A lists military specialties eligible for test programs, App. B gives AFEES test cell assignments, App. C discusses changes in test program effects over time, and App. D gives additional (FY82) information on the Ultra-VEAP kicker.

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[9]The 1981 test included a retention component: special educational benefits offered to reenlistees. This report deals only with the enlistment portion of the test.

II. BACKGROUND

Despite its intention to encourage enlistments, Congress did not establish in VEAP a very attractive incentive. Instead of the $13,140 in maximum educational benefits payable to an individual in 1976 under the GI Bill, VEAP provided for a maximum payout of only $8,-100. Moreover, to receive any benefits under VEAP the individual servicemember is required to make monthly contributions of $50 to $75[1] while in the service, up to a maximum of $2,700, which are later matched two-for-one by the Veterans Administration.[2] Thus, of the $8,100 maximum benefit payout, only $5,400 was paid by the government. In addition, GI Bill benefit levels have been periodically increased by Congress, but the nature of the VEAP benefits—payouts from a fixed fund—effectively precludes such increases for VEAP.

Dramatic though the fall in maximum benefits was, that simple comparison masks the effect of the contribution requirement of VEAP. That requirement makes it impossible to assess unambiguously the relative values of the GI Bill and VEAP,[3] but illustrative present value calculations indicate that the change resulted in a drop of at least 70 percent, and perhaps as much as 95 percent. When discounted to the point of enlistment, the stream of benefit payments from the individual's VEAP fund would appear to be worth very little to the potential enlistee because of the requirement of in-service contributions.[4] This is borne out by statistics on participation in VEAP among the enlisted force, which show that in 1978 fewer than 25 percent of eligible enlistees had ever participated (including those who participated only for short periods), compared with GI Bill usage rates of more than 50 percent for Vietnam era veterans.[5]

Perhaps recognizing that the basic VEAP entitlement might not be a sufficient incentive to attract the numbers of high-ability youths required by the services, Congress also enabled the Department of Defense to enhance the benefits offered to selected enlistees: "The Secretary is authorized to contribute to the fund of any participant such contributions as the Secretary deems necessary or appropriate to

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[1]Since changed to $25 to $100, with lump sum payments also allowed.

[2]Responsibility for providing the matching funds has recently been transferred to the Department of Defense.

[3]Fernandez (1981).

[4]Fernandez (1980), pp. 16-28.

[5]OASD(MRA&L) (1980b), pp. 2-8, 2-9.

encourage persons to enter or remain in the Armed Forces."[6] No use was made of this authority, however, until Congress, in the FY79 Defense Authorization Bill, directed the Secretary of Defense to conduct a test of the attractiveness of increased VEAP benefits and a two-year enlistment option as enlistment incentives for the Army and Navy.

THE MULTIPLE OPTION RECRUITING EXPERIMENT

The experiment conducted during 1979 and early 1980, dubbed the Multiple Option Recruiting Experiment (MORE) by Haggstrom, tested various combinations of enhanced VEAP benefits, a two-year enlistment option, a restriction that participants serve an initial tour in Europe, and, in the Navy, a guarantee of Class "A" training school assignment. Areas served by individual Armed Forces Entrance and Examining Stations (AFEES) were assigned to geographically dispersed "test cells": six (later expanded to eight) in the Army, six in the Navy, and two in the Marine Corps.[7] One cell in each service served as a control, offering no new enlistment options; in the other cells the test options were offered. MORE was scheduled to run for one year, beginning on 1 January 1979 in the Army, 1 March in the Navy, and 15 April in the Marine Corps.

MORE tested two levels of educational benefit enhancements, both in the form of "kickers"—lump sum additions to the individual's VEAP fund. The basic test plan offered kickers of $2,000, $3,000, and $4,000 to recruits enlisting for initial tours of two, three, and four years, respectively. The "Super-VEAP" plan, tested only by the Army and beginning on July 1, 1979, offered kickers of $4,000, $5,000, and $6,000. Both plans were available only to "high-quality" recruits; high school graduates who scored above the 50th percentile on the Armed Forces Qualification Test, the services' basic aptitude test.

The Army conducted the largerst test of the VEAP kickers. The Marine Corps did not test this option at all, and the Navy allocated only 12 percent of the country to kicker programs: 8 percent to two cells offering kickers in conjunction with the two-year option, and 4 percent to one cell offering kickers to four-year enlistees (the standard Navy tour). Furthermore, the Navy discontinued the four-year kickers in mid-June when their goal of 500 recruits under the program was reached. Haggstrom et al. (1981) estimated that the four-year

[6]38 U.S. Code 1622(c).
[7]For a full description of the test, see Haggstrom et al. (1981).

kickers raised Navy enlistments of high-quality males by 4.2 percent, but the standard error of this estimate—10.8 percent—made it of no practical value. The two-year kickers had no apparent effect on Navy enlistments.[8]

Although more ambitious in scope than the Navy test, the Army test of the VEAP kickers was plagued by a similar problem of too small a comparison group. With most of the country given over to the kicker test cells, less than 7 percent remained for the control cell, against which the success of the kicker programs was measured. The basic kicker program appeared to have increased Army high-quality male enlistments by 7.3 percent, but the standard error of this estimate was rather large (5.3 percent), and Haggstrom et al. noted several reasons why they thought that it might be an inflated estimate of the actual response. The estimate of the Super-VEAP kicker effect was even smaller, and also not statistically significant. Finally, although the kicker option was restricted to enlistees in certain "critical skills," Haggstrom et al. found no substantial impact of the kickers on the distribution of enlistees between eligible specialties and the other Army specialties.

In short, the MORE results on the effects of educational benefits were inconclusive. The VEAP kickers appeared to have increased enlistments in the areas in which they were offered, but the increases were small enough that they may have been due simply to randomness in the data. Nonetheless, the test gave the first empirical support for the belief, previously based only on conjecture,[9] that the decline in the quality of enlistees during 1977 and 1978 was attributable, at least in part, to the termination of the G.I. Bill.

MORE PHASE II

Despite the new MORE options, Army recruiting results for fiscal year 1979 were disappointing. Fewer than 26,000 high-quality males accessed,[10] the smallest number of the All Volunteer Force era and barely half the number accessed in fiscal year 1976, the last year

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[8]The only Navy test cell to show a sizeable enlistment response offered a program that was inherently less attractive than the program in the two-year VEAP kicker cells, suggesting that this effect was simply a random phenomenon.

[9]See, for example, Hunter and Nelson (1979).

[10]The term "accession" refers to the commencement of an active duty tour. This is distinct from an enlistment, taken here to mean the signing of an enlistment contract, which may precede the individual's accession by several months. The intervening period is spent in a delayed entry program—technically service in the Reserves.

before the termination of the GI Bill.[11] Partly as a result, the Army test was terminated early, and on 4 December 1979 a new Phase II MORE was begun. The experiment format was retained only for the two-year option, which was extended to cover all but approximately 7 percent of the country. The European restriction was dropped, and the Super-VEAP kicker program, with kicker levels set at $2,000, $4,000, and $6,000, was extended nationwide. The Navy and Marine Corps tests continued uninterrupted, to end as originally scheduled.

With the Army kickers being offered nationwide, and the Navy test of VEAP kickers even more limited than at its inception, the Phase II period from December 1979 to September 1980 could provide no information about the attractiveness of educational benefits. In retrospect, however, it was a most important ten months. Uncontaminated by major experiments with enlistment incentives, unaffected by policy changes concerning such incentives, it was to provide a natural baseline against which to measure enlistment gains produced by the programs tested in the Educational Assistance Test Program (EATP) of 1981. The educational assistance program in place during this period —basic VEAP in the Navy, Air Force, and Marines, plus kickers for qualifying enlistees in the Army only—was to become the control program of the 1981 test.

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[11]The 26,000 figure would later be revised downward to less than 22,000. Norming errors in the scoring of the AFQT, the full extent of which were not understood until 1980, resulted in reported percentile scores that were too high for many recruits between January 1976 and September 1980 (see OASD(MRA&L), 1980a). Earlier tests (1973-1975) may also have been misnormed, but their scoring has not been reexamined.

III. THE EDUCATIONAL ASSISTANCE TEST PROGRAM

ORIGINS

The Army's recruiting problems of 1979 continued unabated in 1980. Although Army accessions of high-quality males rose slightly in fiscal year 1980 over their FY79 levels, greater total requirements drove the proportion of enlistees falling in this category to a record low. Scarcely 52 percent of Army enlistees entered with a high school diploma, and among males the proportion was less than 47 percent.[1] Compounding the problem, a long-awaited DoD report on the misnorming of the AFQT,[2] released in July 1980, revealed that nearly half the Army accessions in fiscal year 1979 fell in Category IV on the AFQT (10th through 30th percentile, the lowest group legally acceptable), rather than the 9 percent previously reported.[3] The fact of even lower recruit quality than had previously been thought fueled the drive for a new GI Bill. Action proceeded on two fronts.

Ultra-VEAP Kicker

Within DoD, the Army pushed for even larger enhancements to the VEAP funds of selected enlistees. Proponents argued that the MORE results indicated that educational benefits could be an effective inducement to enlist, and that those results were inconclusive primarily because the enhancements tested were simply too small to elicit a sizable response. Larger kickers, they argued, would yield a response large enough to be more definitively measured.

The Office of the Secretary of Defense approved a program of larger Army kickers on a test basis in the summer of 1980. The amounts settled upon were $8,000 for two-year enlistees and $12,000 for three- and four-year enlistees. A test cell covering 19 percent of the country was selected (see below), and a starting date of October 1, 1980, established.

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[1]The Army subsequently dropped its requirement that all female enlistees be high school graduates or holders of GED certificates.

[2]OASD(MRA&L) (1980a).

[3]The proportions for the other services, with previously reported figures in parentheses, were: Navy, 18(4); Marine Corps, 26(4); Air Force, 9(0).

Congressional Action

In its report on H.R. 6974, the Department of Defense Authorization Act for 1981, the House Armed Services Committee (HASC) noted:

> The recruiting force needs new tools to attract enlistees. Continual increases in the recruiting and advertising budget miss the heart of the problem. For most youth today, the military no longer holds out an image of providing an opportunity for personal development. The loss of a substantial, non-contributory educational assistance benefit as a concomitant to military service has left the military incapable of reaching a large segment of society. The committee intends to consider legislation shortly that would establish an educational assistance program that, hopefully, will provide a substantial inducement to serve in the military.[4]

Although not prepared at that time to recommend a new educational assistance program, the HASC had approved an amendment to H.R. 6974 which authorized a test program. The amendment was offered on the House floor and promptly approved. The stated purpose of the test program was: "To encourage enlistments and reenlistments for service on active duty in the Armed Forces."[5] Reflecting this limited purpose, the amendment placed responsibility for funding the program with the Department of Defense, rather than the Veterans Administration. A provision limiting eligibility to enlistees or reenlistees during the period October 1, 1980, to September 30, 1981, made the program a test. Eligibility was further limited to "graduates from a secondary school," and the amendment's sponsor, Representative Richard C. White, stated that "this program is to be used selectively to assist recruiting in critical skill areas."[6] Finally, the amendment required the Secretary of Defense to provide quarterly reports to the Congress on the operation of the program.

When H.R. 6974 reached the Senate Armed Services Committee (SASC) the bill was amended in its entirety. Among the provisions eliminated by the amendment was the test program, but the SASC retained the concept by authorizing a one year "pilot test" of three new programs: (1) a program of DoD payment of student loans held by Active and Reserve enlistees and reenlistees; (2) a noncontributory version of VEAP under which the Secretary of Defense was authorized to make the monthly VEAP contributions of enlistees and reenlistees; and (3) a retention plan, under which the Secretary was

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[4]U.S. House of Representatives (1980), pp. 116-117.
[5]Public Law 96-342, Section 901(a); and 10 U.S. Code 2141.
[6]*Congressional Record*, May 15, 1980, p. H3719.

authorized to make payments from a reenlistee's VEAP account to his spouse or children. Each of the programs was authorized for those components and specialties selected by the Secretary of Defense, but the committee noted with regard to the student loan repayment program that it expected "particular attention to be given to combat and combat related skills and other critical skills in which there are shortages."[7] The committee also recommended that the Phase II MORE test then in place "be expanded to include significantly higher levels of Government contributions and shorter terms of enlistment to qualify for the additional payments."[8]

On the purposes of the test, the committee noted that:

> A full program of educational assistance expanded to cover all members on active duty and their dependents, or even selected skills, could prove to be very expensive. But an effective educational assistance program may well be the last hope of the volunteer force to attract personnel of needed quality. So, a test of the effectiveness of these approaches is needed.[9]
>
> This [Noncontributory VEAP] would be a test of a program similar to the old GI Bill where educational assistance would be a benefit of military service and not depend on any contribution from the member at all.[10]

Meeting in conference, the two houses combined their various test programs in Title IX of the bill, which was then passed as Public Law 96-342 (September 8, 1980). The House amendment became Section 901, the loan payment program Section 902,[11] and Section 903 established the Noncontributory VEAP program and its dependent payment provision. Section 904 authorized $75 million to pay for the three programs, and 905 contained a Congressional reporting requirement. The conference committee action, and the SASC recommendation that DoD test larger VEAP kickers (retained in the conference reports), created the Educational Assistance Test Program.

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[7]U.S. Senate (1980), p. 120.

[8]U.S. Senate (1980), p. 120. Note that the Ultra-VEAP kicker test program, then being designed by OSD and the Army, satisfied this provision.

[9]U.S. Senate (1980), p. 121.

[10]U.S. Senate (1980), p. 120.

[11]The loan payment program was implemented nationwide as part of EATP, effectively precluding this "test" from providing any information about the attractiveness of loan repayment as an enlistment incentive. In addition, confusion about the program's eligibility requirements—specifically, what evidence of an outstanding student loan the enlistee was expected to provide—made initially reported counts of program participants unreliable. Beyond uncovering these reporting problems, this study made no attempt to analyze the Section 902 program.

WHY AN EXPERIMENT?

The history of military educational benefits raises an important question: Was a test necessary? Other approaches could be used to estimate how responsive young people are to the services' offers of educational benefits. In particular, the Congressional Budget Office (1982) has estimated the probable effects of four alternative benefit packages by treating educational benefits as simply a deferred form of compensation and, using additional assumptions, applying well-accepted estimates of the effect of a change in first-term pay. This author has used the same approach in the past (Fernandez, 1980).

In this "theoretical" approach, the potential recruit is modeled as a rational economic man. He evaluates alternative future income streams by discounting them to the present and comparing their present values. This approach may be criticized as depending on assumptions about individual discount rates, and expected inflation rates, that have not been empirically supported (and, of course, on the economist's concept of rationality); in addition, several simplifications and assumptions must be made in the model to make it usable. Two assumptions are particularly important, and suspect. First, the approach implicitly assumes that college-bound[12] and noncollege-bound potential recruits are identical in their responsiveness to military pay changes. This allows the use of available estimates of the enlistment response to pay changes, which are based on data for the two groups combined. Second, the approach treats the decision to attend college as exogenous, thus permitting the use of past data on educational benefit usage rates for estimating the size of the baseline number of college-bound recruits, compared with which the incremental effect of a new program is calculated.

A more complete model would recognize that college attendance is not primarily consumption, but rather investment. The rational individual will compare the present values of his or her future income streams with and without college (including nonpecuniary returns, such as a pleasant working environment), subtracting from the former the costs of college attendance, including forgone earnings while in college. Anything that makes college attendance less expensive—military educational benefits, for example—will affect the decision whether to go to college. Thus, that decision is endogenous, determined in part by the benefit program offered, and to estimate the program's probable effect we must estimate the expansion in college

[12]For simplicity, this discussion speaks of college as if it were the only post-military educational option, although in fact other forms of training are available and many qualify the individual for benefits under the EATP and VEAP plans. The considerations discussed apply regardless of the specific form of education.

attendance that it will produce. Further, this model makes it clear that the factors affecting the enlistment decisions of individuals considering college are different from those of individuals who have no such plans. It is quite plausible that the two groups would respond differently to changes in military pay, making inappropriate the use of a pay response estimate based on data for both groups together. In particular, we might suppose that the responsiveness to pay changes of possible college attendees would vary with the availability of non-military sources of educational assistance. This obviously would not be true of individuals for whom the returns to investment in further education would not be positive at any feasible cost of college attendance.

Even were it reasonable to treat the college decision as exogenous, the standard approach of using past benefit usage rates to estimate the size of the baseline college-bound pool appears to be too conservative. What is important in determining the response to an educational benefit program is not the number who actually go to college, but the number who intend to go. Orvis and Hawes (forthcoming), reporting on a survey of military applicants, note that two-thirds of high-quality applicants aspire to graduation from college, and only 14 percent plan no further education. Clearly these intentions are unrealistic, but if they indicate the interest of these people in educational benefits it would help explain why the Congressional Budget Office estimate of the probable response to one of the test programs is substantially lower than the observed response.

That the approach typically used to assess the probable effects of proposed educational benefit programs is flawed does not necessarily mean that theory and available evidence are insufficient to permit more accurate estimation, but in fact that is the case. The simplified model has been used not so much because of a deficiency of theory (although the full theoretical model is quite complex), but because of a deficiency of data. Everything must be pinned on a pay response estimate, which stretches the theory beyond the breaking point. The Educational Assistance Test Program solves both problems at once, sidestepping the need for a fully developed and validated theory, and avoiding the use of possibly inappropriate estimates, by directly yielding indications of how individuals react to specific proposed programs.

TEST PROGRAMS

The separate actions of DoD and the two houses of Congress resulted in three programs to be tested, plus a control program that was

more generous in the Army than in the other services. As was the case with MORE, DoD elected to limit eligibility for the special programs (see below), including the Army's kickers in the control program, although all enlistees remained eligible to participate in the basic VEAP program. Below we first summarize the basic provisions of the four programs, then provide more detailed descriptions.

- *Control program*: basic, contributory VEAP; kickers up to $6,000 in Army only.
- *Ultra-VEAP kicker program*: Army kickers up to $12,000; other services offer basic VEAP only.
- *Noncontributory VEAP program*: Section 903 (Senate); DoD pays individual's VEAP contribution; Army kickers up to $6,000.
- *Tuition/Stipend program*: Section 901 (House); tuition assistance ($1,200/year) plus subsistence allowance ($300/month); benefits indexed for inflation; may be transferred to dependents or cashed out upon reenlistment; same benefits in all services.

Army Programs

The Army's kickers—both in the Ultra-VEAP test program and in the Super-VEAP control program—were *enhancements* to the benefits of VEAP. Thus, qualifying for a kicker by a virtue of satisfying the eligibility criteria was not synonymous with earning the kicker. To earn any portion of a kicker, the individual was required to participate in basic VEAP, through monthly contributions of $25 to $100, for at least 12 months (this is also the minimum participation period to qualify for the two-for-one matching of basic VEAP). Subsequent months of participation earned the enlistee additional portions of the kicker, as shown in Table 1. The amount of the monthly contribution did not matter, so that a two-year enlistee who qualified for an $8,000 kicker could, for example, contribute only $600 (the minimum $25 per month for 24 months) and leave the Army with an education fund of $9,800. If he chose to contribute $100 per month, however, and discontinued participation after 12 months, his fund would total only $8,000 ($4,400 kicker, $1,200 contribution, and $2,400 matching).[13]

Just as qualifying for a kicker did not mean earning it, so earning a kicker does not necessarily mean receiving it. After leaving the service, the individual may draw upon his fund only while pursuing an

[13]Lump sum contributions to the VEAP fund are treated as if they were made in equal monthly installments of $75.

Table 1

EARNING RATES FOR VEAP KICKERS
(In dollars)

Kicker amount	2,000	4,000	6,000	8,000	12,000
Earned after 12 months	1,600	2,600	3,600	4,400	4,800
Earned for each succeeding month	100	100	100	300	300

Source: Headquarters, U.S. Army Recruiting Command (1981), p. 31.

approved program of training or higher education (restrictions similar to those of the GI Bill apply). Payouts from the fund are made in equal monthly installments, the amount being the starting amount of the fund divided by the number of months of the individual's in-service participation in VEAP. Thus the servicemember in the first example above, who contributed $25 per month for two years, would receive $408 per month for up to 24 months (almost three academic years). If he chose to attend school for only two years (18 months), he would forfeit $2,300 in kicker plus matching funds (the remainder of his contribution, $150, would be returned to him if he requested it). The second individual ($100 monthly contributions for 12 months) could receive payments only for 12 months, but his monthly payment amount would be $667![14]

Noncontributory VEAP

The Noncontributory VEAP (NCV) program, as implemented in EATP, was a simple modification of the control program of the test. As the control program included kickers, in the Army only, of $2,000, $4,000, and $6,000, so did the NCV program. In all four services, enlistees satisfying the EATP eligibility criteria (see below) could under NCV have their individual contributions to the VEAP fund paid by the Defense Department.[15] Once enlisted, the servicemember eligible for NCV was required to participate in VEAP, but such participation would be virtually automatic.

[14]Thirty-six is the maximum number of payment months allowed. Servicemembers who contribute for a longer period are paid as if they had accumulated their funds over 36 months.

[15]Note that the two-for-one matching funds of basic VEAP were paid by the Veterans Administration at the time of the test.

DoD established its contribution rate under NCV at $75 per month, thus implicitly requiring three years of service to accumulate the maximum $8,100 VEAP fund. This also fixed the payout rate at $225 per month for Navy, Air Force, and Marine Corps enlistees, and at $308, $336, and $392 for Army enlistees serving two-, three-, and four-year tours, respectively. Enlistees in the Navy, Marines, and Air Force who did not complete their obligated tours would receive the same $225 per month, although only for the number of months they served, and provided that they met the minimum VEAP participation requirement and did not receive a dishonorable discharge. Army enlistees failing to complete their tours would also have their payment periods proportionately reduced, but because of the front-loading of kicker earning (see Table 1) would receive somewhat larger payments each month than had they served their full tours.

Tuition/Stipend

The Tuition/Stipend (T/S) program was quite unlike the other programs tested in virtually every respect except the eligibility criteria. First, it was not based on VEAP. Indeed, enlistees who qualified for the T/S program were not eligible to participate in basic VEAP nor, if they were entering the Army, in a VEAP kicker program. Second, the monthly benefit amount under T/S did not depend upon the individual's participation or contribution decisions while in the service. T/S required no individual contributions, and was not based on the accumulation of a fund. Third, the T/S benefit amount was indexed: "Once each year, the Secretary of Defense shall adjust the amount of educational assistance . . . in a manner consistent with the change over the preceding twelve-month period in the average actual cost of attendance at public institutions of higher education."[16] Fourth, the T/S program contained two provisions designed to minimize the negative reenlistment effect of postservice educational benefits: upon reenlistment, the servicemember is entitled to (a) receive a lump sum payment (in lieu of educational assistance) equal to 60 percent of his earned entitlement, or (b) transfer all or part of his earned entitlement to his spouse or dependent child. Finally, earning of any benefits under T/S requires two years of service, and one academic year (nine months) of benefits is earned for each 12 months of service.

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[16]10 USC 2145(a). Almost unique among indexed federal programs, this one is tied to an index related to the purpose for which the funds are provided, rather than to the Consumer Price Index.

Benefits payable under T/S take two forms: educational assistance and subsistence allowance. The first, initially set at a maximum of $1,200 per academic year, was authorized for payment of "those educational expenses normally incurred by students at the institution involved,"[17] including tuition, fees, books, laboratory fees, and shop fees. Thus, veterans attending relatively inexpensive schools would not receive the maximum amount.[18] The subsistence allowance, initially set at $300 per month, does not depend on the costs of the particular institution attended.

The earning rates under T/S mean that four years of active duty are required for a servicemember to earn the maximum benefit. Under all the other test plans, and under basic VEAP, four academic years of benefits could be earned during a three-year tour (the Army's Super-VEAP kicker program does require four years of service for the maximum benefit, but the fourth year adds only $2,000 to the total). Because of this provision of T/S, an Army recruit enlisting for a "standard" tour (three years) would earn less than his counterparts in the Navy and Air Force, where the standard tour is four years.[19]

The Army was further hurt under the T/S program, compared with other services and with its position under the control program, in that it could not offer any special enhancements to the benefits. The legislation establishing T/S did authorize differential benefits—"depending on the needs of the services, different amounts may be established for different categories of persons or enlistments"[20]—but this applied only to the educational assistance portion and not to the subsistence allowance. The authority was not used when the details of the test program were set.

Benefit Amounts and the Value of Benefits

Table 2 shows the maximum benefit amount, by tour length and service, for each of the test programs and the control program. Servicemembers' contributions are netted out for the contributory pro-

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[17]10 USC 2143(a)

[18]Charges for tuition and required fees at public institutions of higher education averaged $595 per year in 1979-80. The average for private institutions was $3,108 (National Center for Education Statistics, 1981).

[19]It should be noted that in many of the larger Army specialties eligible for the the EATP programs, many enlistees choose a four-year tour to take advantage of the Army's Cash Bonus Enlistment Option. Unlike the test programs, however, the bonus option was offered nationwide in both fiscal years 1980 and 1981. The bonus option is discussed more fully below.

[20]10 USC 2143(b)(1).

Table 2

MAXIMUM NET BENEFITS AND BENEFIT MONTHS FOR EATP PROGRAMS

(In dollars)

Program/ Service	Tour Length		
	2 Years	3 Years	4 Years
Control			
Army	6,800[a]	9,400	11,400
Navy, Air Force	—	—	5,400
Benefit months	24	36	36
Ultra-VEAP kicker			
Army	12,800[a]	17,400	17,400
Navy, Air Force	—	—	5,400
Benefit months	24	36	36
Noncontributory VEAP			
Army	7,400	12,100	14,100
Navy, Air Force	—	—	8,100
Benefit months	24	36	36
Tuition/Stipend			
Army	7,800	11,700	15,600
Navy, Air Force	—	—	15,600
Benefit months	18	27	36

[a]Based on individual contributions of $100 per months. Maximum benefits for longer tour lengths can be earned with smaller monthly contributions.

grams. The table also indicates the maximum number of school months over which the benefit payments may be received.

Simple comparisons of the maximum benefits, such as those shown in Table 2, give a misleading impression of the relative values of the programs to a potential enlistee. The flexibility of the contributory VEAP programs (including the Army kicker programs), noted above, should in principle increase their worths relative to those of the two noncontributory programs. The servicemember who plans to attend school for only two years, for example, can choose to structure his contributions and participation under the control and Ultra-VEAP Kicker (UVK) programs so as to receive *more* than half of the maximum benefit available to an enlistee who attends school for four

years. The advantage resulting from this flexibility, however, is likely to be more than outweighed in the mind of the thoughtful recruit by the loss and deferral of income that results from the contribution requirement of VEAP. No interest is paid on the individual's VEAP contributions, but even if it were, the deferral of income, combined with the noted impatience of youth, would seem to reduce the values of the contributory programs by more than the mere netting out of servicemembers' contributions would suggest.

Table 3 adjusts the data in Table 2, showing illustrative calculations of the values of the various programs to the potential enlistee *at the time of enlistment*. Future benefit receipts, and the contributions under the control and UVK programs, are discounted to the enlistment point under the assumption that potential enlistees implicitly

Table 3

ILLUSTRATIVE PRESENT VALUES OF MAXIMUM BENEFITS UNDER EATP PROGRAMS

(In dollars)

Program/ Service	Tour Length		
	2 Years	3 Years	4 Years
Control[a]			
Army	2,902	2,756	2,778
Navy, Air Force	—	—	786
Ultra-VEAP kicker[a]			
Army	6,096	5,942	4,769
Navy, Air Force	—	—	786
Noncontributory VEAP			
Army	3,940	4,819	4,680
Navy, Air Force	—	—	2,689
Tuition/Stipend[b]			
Army	5,745	7,374	8,422
Navy, Air Force	—	—	8,422
Tuition/Stipend (no indexing)			
Army	4,400	5,056	5,178
Navy, Air Force	—	—	5,178

[a]Assumes equal monthly individual contributions over entire tour, to yield maximum possible benefit.

[b]Assumes individual expects annual inflation in education cost index of 8 percent.

apply a nominal discount rate of 20 percent.[21] This discount rate is probably conservative in the sense that, compared with higher rates, it tends to minimize the differences in value between the contributory programs and the noncontributory programs.[22] Assuming a higher discount rate would result in the greatest proportional reduction in value for the control program. Because the comparisons of program values are sensitive to the discount rate assumed, the values reported should be taken only as indicative of the programs' relative worths. The rank ordering of the programs, however, would not be altered by reasonable changes in the assumed discount rate.

Examination of the table reveals several important points. First, the Army's advantage in the control and UVK programs is even greater than a comparison of undiscounted benefits would indicate—approximately twice as great. In present value terms, the Army's kickers give it more than a three-to-one advantage over the other services in the control program, and a six-to-one advantage in the UVK program. Second, eliminating the contribution requirement has a dramatic effect on the value of the control program. The effect is more marked for the Navy and Air Force than it is for the Army because the values of the Army's kickers are not affected by the elimination of contributions.[23] Third, with the exception of the Army's two-year tour, every comparison of the UVK and T/S programs favors the latter when the effects of indexing are included. Again, however, a comparison of T/S with the control program shows T/S to be worth at most three times as much for the Army, but ten times as much for the Navy and Air Force. Finally, the indexing provision of T/S substantially increases its present value. Results from a survey of April 1981 military applicants suggest, however, that potential recruits may not place much value on such a provision.[24] It may be more appropriate, therefore, to use the no-indexing numbers in forming hypotheses about the likely response to the T/S program.

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[21]That is, they view one dollar to be received one year from today as worth only $0.80 today. The "nominal" discount rate incorporates both the rate of time preference (impatience) of the individual and his expectations about price inflation, which reduces the purchasing power of his future benefits.

[22]Cooper (1977), for example, used a nominal discount rate of 20 percent to value first-term pay during a period of lower price inflation than today; Grissmer (1974) used 30 percent.

[23]The apparent anomaly of the smallest increase in present value occurring for the two-year enlistment tour arises because the control program value for that tour length assumes monthly contributions of $100, which yield a greater fund value than does the NCV rate of $75. For three- and four-year tours, the VEAP fund reaches its maximum allowable value in both programs.

[24]Orvis and Hawes (forthcoming).

ELIGIBILITY CRITERIA

Participation in basic (contributory) VEAP has been, since 1977, open to all enlistees. All of the test programs, however, and the Army kickers that were part of the control program, were open only to certain nonprior service recruits. Two criteria for eligibility were imposed: (1) the recruit must have been of "high quality" in terms of educational attainment and mental aptitude and (2) he must have enlisted in one of a set of specified "critical skills."

High Quality

EATP followed the most common current usage of "high quality" in restricting eligibility to possessors of high school diplomas (GED certificates not acceptable) whose scores on the AFQT placed them in test groups I through IIIA (50th percentile or above). This criterion is the same as that previously applied in MORE. It is a more stringent definition of quality, however, than is implicit in the services' enlistment bonus programs, which are also open to high school graduates in test group IIIB (31st through 49th percentiles).

The criterion has its origins in the belief that educational benefits would appeal primarily to brighter, college-bound youths; the desire in DoD to minimize program costs through targeting; and the demands of the services for high-scoring high school graduates. Apart from the technical sophistication of modern weapons and support systems, the services' demands are based on the long-held belief that high school graduates are more likely than nongraduates to complete their initial enlistment tours,[25] and on the belief, only recently given firm support,[26] that scores on the AFQT are good predictors of ability to perform on the job.

The percentages of nonprior service enlistees accessed during fiscal year 1980 who met this criterion, by service and sex, are given in Table 4.

Critical Skills

Each of the services promises at least some of its recruits training in a specific military specialty or group of specialties. These promises

[25]See Buddin (1981) for recent evidence.
[26]Armor et al. (1982).

Table 4

FY80 High-Quality Enlistees by Service and Sex: Percent of Total within Category

Service	Males	Females	Both Sexes
Army	16.6	23.3	17.5
Navy	41.6	35.5	40.9
Air Force	51.0	49.4	50.7
Marine Corps	31.2	73.0	33.5
DoD	31.1	35.6	31.7

are part of the recruit's enlistment contract. If the individual does not like the job or jobs he is offered, he need not enlist, but most individuals who get to the point of discussing specific jobs—at the Armed Forces Entrance and Examining Station (AFEES), after all testing is complete—do in fact enlist. The Army promises specific jobs to all of its recruits, the Navy and Air Force to about two-thirds,[27] and the Marine Corps promises training in one of a set of related specialties.[28] Naturally, all four services find it easier to convince recruits to serve in some specialties than in others.

There is no single set of military jobs that is always defined as "critical skills." In general, a job is considered a critical skill at any given time if it is: (a) hard to fill, (b) understrength, (c) particularly important for combat readiness, or (d) all of the above. An additional consideration that drove the selection for EATP eligibility of many Army jobs was (e) hard to fill with high-quality enlistees.

Table 5 summarizes the types of jobs that were eligible for EATP benefits and the approximate percentages of FY80 high-quality enlistees who entered those jobs, for the Army, Navy, and Air Force. Complete lists of the eligible jobs appear in App. A. The much greater

[27]Navy recruits who are not promised specific job training are promised one of a number of general detail specialties—Seaman, Airman, Fireman—and receive their job training on shipboard rather than in school. Unassigned Air Force recruits are given training assignments during basic training.

[28]This feature of the Marine Corps' job promise led that service to conduct its test in a manner very different from that of the other services. Marine recruits could not be assured at the time of their enlistment that they would be eligible to receive benefits under the test programs. This study did not attempt to measure the effects of the test programs on Marine Corps enlistments because it was felt the results would be too difficult to interpret to be of any practical value.

Table 5

NATURE AND COVERAGE OF EATP ELIGIBLE SPECIALTIES

Service	Types of Specialties	Coverage[a] (%)
Army	Primarily combat arms	57
Navy	Technical	10
Air Force	Mixed	18

[a]Percentage of FY80 high-quality male enlistees who entered EATP eligible specialties.

extent of coverage of the Army jobs than those of the other two services arose from a deliberate policy decision within OSD. The funds allocated for the NCV and T/S programs by Congress were insufficient to permit a large-scale test in all of the services, so the decision was made to test a widely available benefit in only one service, and more limited programs in the others.[29] The Army was selected for the large-scale test because it was having the greatest recruiting difficulties, and because the previously authorized Ultra-VEAP kicker test had already been designed to cover a large number of Army jobs.

The jobs selected by each of the services reflect their different needs and problems. In the Navy, most of the jobs were technical (with the notable exception of the Signalman rating), and several were in the Nuclear Field. The Air Force jobs were more mixed in character, including security guards (accounting for about half of all openings), aircraft munitions handlers, and Morse radio operators. Eligible jobs in the Army were primarily in the ground combat arms, with a small number of jobs—infantryman is the largest of these—accounting for most of the openings.

The Army expanded the list of jobs eligible for EATP benefits considerably while the test was in progress, late in February 1981. The

[29]Alternatively, the period of the test could have been shortened, or the proportion of the country involved reduced, but both would have reduced the reliability of the estimated response rates.

added skills were largely not combat related, including Administration Specialist (clerk/typist), Unit Supply Specialist, Motor Transport Driver, and Cook.[30] While complicating the analysis of the Army portion of the test, this addition did provide new information on the skill channelling effects of educational benefits into combat versus noncombat jobs. In what follows, we refer to the initial set of specialties as "Group I" and the added set as "Group II."

In each of the services, enlistees choosing many of the EATP-eligible specialties were also eligible for cash enlistment bonuses of $1,000 to $5,000, depending on the service and specialty. Appendix A indicates those EATP specialties that also offered bonuses. In addition to the quality (see above) and specialty requirements, the bonus programs also require longer enlistment tours: a minimum of four years in the Army, five in the Navy, and six in the Air Force (normal first tours are three, four, and four years, respectively).

Despite the overlap in eligibility criteria, the bonus programs should not have appreciably affected the test results. With the exception of the Air Force program, which began in FY80, these bonus programs were of long standing, begun well before the test and continuing after it. The bonus options were permanent parts of the services' enlistment packages, equally available in all parts of the country. Finally, the services have viewed bonuses as a tool for filling certain specialties and eliciting longer commitments, rather than as enlistment incentives. As a result, bonuses have not been advertised or heavily promoted by recruiters.

TEST DESIGN

The EATP test design followed the example of MORE in assigning each of the test programs to a geographically dispersed test cell. Rand participated in the test design by developing the assignment algorithm and balancing criteria, collecting relevant data, writing a computer program to generate cell assignments, and producing several alternative sets of assignments. Selection of the final assignment set was made by Rand in consultation with MRA&L.

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[30]Interestingly, many of the added jobs had in the past attracted recruits of even lower average quality than had the traditionally "critical skill" combat arms jobs.

Four basic considerations guided the selection of the test cell areas:

1. The 67 areas served by individual Armed Forces Entrance and Examining Stations were the units to be assigned to cells. Although this forced the splitting of some of the services' recruiting districts between two test cells, it ensured uniformity of the test cells across all four services, and could be more easily implemented than if one service's district boundaries, or some arbitrary unit such as states, had been chosen.
2. All the test cells, and the control cell in particular, would be large. Partly because of the experience with MORE, the cell sizes were chosen with the explicit criterion that subsequent analysis of the test should be able to yield unequivocal statements about whether the test programs had, indeed, raised enlistments. The control program was allocated 51 percent of the country (based on prior enlistment levels of high-quality males), and the UVK, NCV, and T/S cells 19, 15, and 15 percent, respectively.[31]
3. To facilitate advertising, and to minimize disruptions to the services' recruiting programs, certain AFEES were tied together. The AFEES in New York City and in Newark, New Jersey, for example, could not be in different cells because advertising in New York would certainly be seen by those living in New Jersey; the Seattle and Spokane AFEES were linked because they were served by a single Army District Recruiting Command.[32]
4. Assignment of AFEES to test cells would be random, subject to certain balancing criteria to ensure that the areas assigned to each cell had, on average, similar recruiting conditions to those in the other cells.

Balancing Criteria

Longitude and Latitude. To prevent any test cell from being concentrated in one area of the country—the South, for example—the

[31]The larger size of the UVK test cell was, at least in part, a historical accident, arising because the UVK test had already been authorized and designed when Congress mandated a test of two new programs. Budgetary considerations forced the NCV and T/S cells to be somewhat smaller. Given the sizes of the four cells and the test designers' assumptions about the statistical distribution of enlistments (see Haggstrom et al., 1981), an observed enlistment effect of 6 percent for any test program would be large enough to reject the null hypothesis of no true effect.

[32]Not all linkages suggested by recruiting boundaries could be included.

cells were balanced on their mean longitudes and latitudes.[33] No cell differed from the average of the others by more than one degree of longitude or latitude. This criterion did not prevent some cells from being concentrated in the center of the country, and others on the edges, but the assignment set that was selected did not exhibit this problem.

Local Labor Market Conditions. The cells were balanced on the average levels of two labor market variables commonly believed to affect enlistments—unemployment rates and wage rates—during the period October-December 1979.[34] On both of these criteria, the difference between the value for any one cell and for the rest of the country was never more than 2 percent of the nationwide average value.

Enlistment Rates. Past enlistment rates were measured by the ratio of total DoD high-quality male enlistments in 1979 to the 17 to 21 year old population of high-quality Qualified Military Availables (QMA).[35] The value of this variable in each cell differed from the value for the rest of the country by no more than 4 percent. The cells were not specifically balanced on past enlistment rates for the individual services, but with few exceptions they were as closely matched for each of the services as they were for DoD as a whole.

There obviously are other factors that are known or believed to affect enlistments on which, in principle at least, the test cells might have been balanced. Had the enlistment rate variable been excluded from the balancing criteria, there would be a legitimate concern that the cells might have differed greatly in, for example, the basic propensity of their youthful residents to enlist. The effects of such unmeasured variables are captured in the aggregate, however, by the enlistment rate variable, ensuring at least approximate balance on all the possible criteria that were not explicitly included.

Test Cells

The test cell assignments are displayed in Fig. 1, and the AFEES in each cell are listed in App. B. The various constraints imposed on the

[33]All means discussed in this section are weighted means, the weights being the proportions of total DoD high-quality male enlistments in 1979 coming from the particular AFEES. Outlying regions—Alaska, Hawaii, Puerto Rico, and Guam—were not included in the design, but were later assigned to the control program.

[34]These variables were the unemployment rate for all workers 16 years and older, and the average hourly wage rate for production and nonsupervisory workers in manufacturing industries. The rates used were estimated for the AFEES areas from state data, in a manner similar to that described in Sec. IV.

[35]See Huck, Crewes, and Sica (1978) for a discussion of the procedures used to estimate this population. The Defense Manpower Data Center supplied the QMA data.

Fig. 1—EATP test cells

design—in particular the AFEES linkages—did not permit perfect geographic balance, but each cell includes areas in the industrial Northeast, the South, the Midwest, and the Far West. The NCV cell appears to be overrepresented in the Southwest, but this area accounts for less than one third of the cell's total enlistments. The geographic dispersion exhibited by each cell is particularly important, for it means that estimates of the program effects are not likely to be unduly influenced by single events—a severe winter in the Midwest, a housing boom in the sunbelt—that cannot be adequately controlled for in the test analysis.

More generally, the random assignment procedure that underlay the design ensures that such imbalances across the test cells as do arise, either because of factors that were not explicitly balanced or because of changes in recruiting conditions that could not be anticipated, can be presumed to have been generated by processes that are independent, in the statistical sense, from those that generated the test design. This means that even simple comparisons of enlistment levels across the cells, or comparisons of gains relative to some base period, will yield *unbiased* estimates of program effects. Controls for exogenous influences introduced in a more complicated statistical

model will, to the extent that imbalances do arise, change the estimates. The primary reason for introducing them, however, will be to reduce the unexplained variance in enlistments, and therefore to improve the precision of the response rate estimates by reducing their standard errors.

Advertising and Recruiting

A new program cannot be expected to attract substantial numbers of recruits if its existence is not widely known. For this reason, MRA&L authorized considerable advertising of the programs as part of the test design. An Army plan to spend approximately $1.5 million in the UVK cell to introduce the new kickers was authorized, and the Joint Recruiting and Advertising Program (JRAP) was directed to spend like amounts for advertising of the NCV and T/S programs. No separate service advertising of the latter programs was permitted, nor was an advertising approach that sold the options as "available for a limited time" allowed.

Table 6 presents the reported expenditures of JRAP and the Army on advertising of the test programs, including advertising of the Army's Super-VEAP kickers that were part of the control program. These expenditures were *in addition* to the general advertising dollars that would otherwise have been spent in each of the cells.[36] As a result, response estimates derived from the test will combine the effects of the programs themselves with those of their associated advertising. The concluding portion of this section discusses this issue more fully.

Recruiting practices were similarly constrained by MRA&L:

> No unprogrammed increases in recruiters, recruiters aides or helpers, or AFEES guidance counselors or liaison NCO's will be made. Recruiter incentives, evaluation and quota assignments must be consistently applied across all test cells and the control cell. Control cell recruiters should in no way be penalized in the recruiter incentive/award system.[37]

The data we obtained for this study on numbers of recruiters do show unequal changes across cells between 1980 and 1981 (see Sec. IV), but there is nothing in them to indicate that recruiter assignments were deliberately manipulated to influence the test results.

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[36]The Army expenditures were taken from their previously authorized nationwide budget, not specifically from planned expenditures to the control and UVK cells.

[37]EATP implementing memorandum from the Assistant Secretary of Defense (Manpower, Reserve Affairs, and Logistics), 1980.

Table 6

PROGRAM ADVERTISING EXPENDITURES BY CELL
(In dollars)

	Total (millions)	Per Capita[a]
Army		
Control	1.12	0.20
UVK	1.59	0.77
JRAP		
NCV	1.42	0.80
T/S	1.56	0.92

[a]Total divided by 17-21 year old male population.

WHAT CAN THE TEST SHOW?

The Educational Assistance Test Program was not a single test. Many actors were involved in the design of the experimental programs, among them the Army with its desire for an immediate expansion of benefits, the House with its plan for a new GI Bill, and the Senate Armed Services Committee with its concern about the contribution requirement of VEAP. The result was not one test, but three. The test measures three distinct, and distinctly different, programs against a "control" program that represents simply the status quo ante: the results of direct comparisons among the test programs are difficult to interpret.

Tuition/Stipend Program

The most complex test "treatment" was the Tuition/Stipend program. Compared with the control program, it offered: (a) an arguably simpler program, (b) more generous benefits, (c) a two-type benefit (tuition assistance and subsistence allowance) that may have its own appeal, (d) an indexing provision, and (e) the same benefit for all the services. If we observe that the T/S program produces more enlistments than the control, to which of these provisions are we to attrib-

ute the increase? The test of the Tuition/Stipend program may yield some general information on the effects of a more generous program, and perhaps some evidence on the effect of equalizing benefits across the services, but it will not tell us which of the specific features of the program should appear in any replacement for VEAP.

Greater Benefits

The other two programs may yield results with more ready interpretations. If we observe that the UVK program increases Army enlistments, it means we have finally found support for the belief, so widely held but based on little clear evidence, that educational benefits can be an effective enlistment incentive. The ending of the GI Bill did not provide this evidence, nor did the Multiple Option Recruiting Experiment. Of course, we must be sure that the Army's gain did not simply come at the expense of the other services, but this we can learn by comparing the other services' enlistments in the UVK cell with their enlistments in the control cell, remembering that their offerings did not differ between these two cells.

Contributions

A substantial increase in enlistments in the NCV cell, relative to the control cell, would suggest that the contribution provision of VEAP has indeed limited its attractiveness. Statistical significance alone, however, is not enough; the control and NCV programs do not differ only in this one respect. Under the NCV program, DoD replaces the individual's VEAP contributions with its own, so the net amount of benefits under NCV is 50 percent larger than under basic VEAP. We must thus find a large increase—larger than the greater net benefit alone would explain—to conclude that eliminating the contribution requirement would increase VEAP's appeal. Before using even this result as a reason to eliminate contributions, however, we might wish to examine the costs: VEAP limits costs by discouraging from participation those enlistees who are only marginally committed to higher education. A moderately attractive contributory program may be much more cost effective than a noncontributory program that brings in many more recruits.

If we do not observe a substantial increase—or rather, if we observe a response that we can be sure is not large (given the uncertainty of

statistical estimates)—it would strongly suggest that the contribution requirement is not the culprit in VEAP's supposed failure. Of course, a poor showing for the Noncontributory VEAP program might simply mean that it was not sold effectively. Before concluding that dropping the contribution requirement would not improve the effectiveness of VEAP, therefore, we would want to find other evidence to support the test result.

Effects on Different Groups

The gross enlistment responses discussed thus far may usefully be subdivided along several lines: male versus female, high-quality versus lower-quality, eligible specialties versus ineligible. In principle, we should be able to observe effects on enlistments of each of these groups, although in some cases the effects may be too small, or the numbers involved too few, for the effects to be statistically different from zero.

The nature of the eligible specialties, particularly in the Army, suggests that it may be easier to observe effects of the test programs among males than among females. The bulk of Army openings in eligible specialties were in the combat arms, and therefore specifically closed to women. Females are also not allowed in several of the Navy specialties. Although this information would not have appeared in the program advertising, it certainly would have been clear to recruiters, the primary source of information for most potential recruits about opportunities in the military. In addition, the services have had much less difficulty meeting their quotas for female recruits, so any potential response among females to test programs would probably be dulled by the effects of the services' demands. For both these reasons, the analysis of this study will examine enlistments of males only.

A focus on high-quality enlistments is even more natural than a focus on males. This is the group that is eligible for the test programs; this is the group that any educational benefit program, whether specifically restricted to high quality or not, is designed to attract. Nonetheless, effects on lower-quality enlistments are possible. Such effects are of interest both because of their cost implications for an unrestricted program and because lower-quality recruits who are initially attracted to the military by the promise of educational benefits may be more likely to complete their tours than equally qualified recruits who enter for other reasons. Two forces will be at work on lower-quality enlistments, however, and because they should work in opposite directions the net effect to be expected is unclear. On the one hand, the potential recruit will not know of all the quality restrictions

until he has spoken to a recruiter, and the recruiter will not know whether an individual is qualified for the test programs until his aptitude test results are available. By this time, the individual may already have been sold on military service, or if educational benefits are particularly important to him, may be convinced by his recruiter that basic VEAP is sufficiently generous. On the other hand, if a test program substantially increases high-quality enlistments it may dry up opportunities for lower-quality individuals, either directly through the limitations imposed by the services' total requirements, or indirectly by limiting the jobs available for lower-quality recruits.

Skill Channelling

The test programs were designed to serve two purposes: (1) to bring more high-quality recruits into the force and (2) to move high-quality recruits into the specified hard-to-fill specialties. The first is the enlistment effect of a program, the second the skill channelling effect. We may observe either one without the other, but it is more likely that both will occur.

It might be thought that the test programs should cause enlistments in the eligible jobs to rise by at least as large a number as total enlistments. Every new enlistment induced by the program would be in the eligible specialties, and some of those enlistees who would have entered had there been no program will find the program sufficiently attractive to change their job choices from what they would otherwise have been. This argument ignores, however, the institutional realities of the enlistment process, and implicitly assumes that every recruit whose enlistment is attributable to the program enlists solely to receive the benefits it offers.

Discussions about specific jobs do not take place until very late in the enlistment process; typically, immediately before the enlistment contract is signed. The recruiter, who starts the process, is expected to sell the potential recruit on entering the particular service, but he is not supposed to discuss specialties. That is the job of the guidance counselor (the Army term, but all the services have similar positions), who sees the individual at the AFEES after all testing is complete. Sitting before a computer terminal tied into his service's job reservation system, the guidance counselor can determine for which jobs the individual is qualified and when there is a training slot open for each of them. The individual who has been initially attracted to the service in part by the availability of one of the EATP programs will first learn about the specific eligible specialties in his discussion with the guidance counselor.

The individual who is considering military service solely to receive the benefits of one of the EATP programs presumably will, when presented with the information on eligible specialties, decide either to enlist in one of them or to go home. Probably more common, however, will be the individual for whom the program is only one of a number of factors affecting his enlistment decision. Advertising about the program, or initial discussions about it with a recruiter, may have made him more receptive to the recruiter's general sales pitch, but he is also attracted by other features of military service. Indeed, although the program was what initially attracted him—perhaps even to the extent that he would not have talked to a recruiter had he not heard of the program—it may not be the most important factor by the time he talks to the guidance counselor. For this individual, the final decision is more complex. If he chooses an ineligible specialty, he is at least eligible for benefits under VEAP, and he may expect to spend his three or four years of service more comfortably or in a more interesting job. If the EATP program is not as important a consideration in his enlistment decision as it is for the first individual, neither should it be as important in determining his specialty choice. It is by no means clear, therefore, that skill channelling will be complete even among those additional recruits whose enlistments can be attributed to the test programs.

This discussion suggests that it may not be necessary, or even advisable, to limit the analysis of the test programs' effects to enlistments in eligible specialties. Certainly the gains in enlistments in those specialties should be examined, but there may well be even larger gains in total enlistments. Moreover, if programs targeted on critical skills, such as those tested in the EATP, can be found to elicit a sizable total enlistment response, it would be an even more important finding than that they can cause skill channelling. A significant enlistment response to the small Navy and Air Force programs, in particular, would suggest that an educational benefit program that was targeted on a small number of specialties could be very cost effective. Economic rents, the costly bane of all general entitlement programs, would be avoided.

Man-Year Effects: Tour Length and Retention

The Army's unique tour-length flexibility—high-quality recruits may choose among tours of two, three, and four (or more) years, each with its own set of incentives—opens the possibility that each of the test programs may have affected the tour-length decisions of eligible recruits. Retention effects are possible in all of the services: it is

generally recognized that postservice educational benefits provide a disincentive to reenlist (see, for example, Congressional Budget Office, 1982). Both of these effects could lead to a change in the average numbers of man-years contributed by recruits under the test programs, compared with the number under the control program. Although the data available for this study did not indicate tour length, and any retention effects cannot be observed for several years, the force manning consequences of these potential effects are important enough that they clearly deserve future examination.

As Table 2 shows, each of the test programs offered a different pattern of benefit levels across tour lengths and a different pattern from the control program. In terms of present value (Table 3), some programs offered most to two-year enlistees; only one, the Tuition/Stipend program, provided an incentive for the recruit to choose a four-year tour. Even had the relative values of different tour lengths been the same in the test programs and the control program, however, the test programs might still have affected the choices of recruits because the base level of returns to military service, to which the benefits were an increment, varies by tour length. The force-manning consequences of changed tour lengths are even greater than simple man-year calculations would indicate. Assuming six months in training, two-year enlistees contribute 40 percent fewer working months, and four-year enlistees 40 percent more, than do three-year enlistees. Moreover, the test programs could have affected the tour length decisions of all eligible enlistees, not merely those whose enlistments are attributable to them. Thus, if a program that increased enlistments caused a substantial shift toward shorter tours, it could lead to a reduction in working months obtained. Similarly, even a program that reduced enlistments could increase working months if it induced more enlistees to choose four-year tours. Which effect was more prevalent, or whether the test programs affected tour-length decisions at all, are questions that can only be answered empirically.

Like tour-length effects, retention effects are not limited to those recruits whose enlistments are attributable to the test programs. Every high-quality recruit in an eligible specialty is given an incentive not to reenlist, unless he really has no interest at all in further education. This means that not only the *proportion* of recruits reenlisting, but even the absolute *number*, may be reduced by more generous programs such as those tested. For example, if a program raised enlistments by 10 percent, but reduced the reenlistment rate from 20 percent to 17 percent, it would reduce the number reenlisting by more than 6 percent.

A thorough examination of man-year effects would consider not only working months contributed by first and second termers, but also

the quality of performance during those months. Presumably, more experienced soldiers are more valuable, both because they are able to perform technical tasks better and because they are more able to assume supervisory responsibilities. Haggstrom et al. (1981) examine this issue and the general man-year problem in the context of a simple model of enlisted retention using assumed retention effects of the MORE options. At present, we do not have empirical data to address these questions fully, but more exact analyses will be possible as data on actual retention and tour-length effects become available.

Social Representativeness

Part of the call for a new GI Bill rests on the premise, as yet unsupported by empirical evidence, that it would attract more college-bound, middle-class youths into a force that allegedly is now composed disproportionately of the poor in general and minorities in particular.[38] If social representativeness in the force is an important goal of military manpower policymakers, it would be important to know whether that goal can be met in an all-volunteer environment through the use of such incentive programs as educational benefits. The Educational Assistance Test Program, however, was not designed to answer that question.

The principal defect of the EATP as a tool for examining social representativeness is the size of the various test cells. The cells were made large enough for the test to yield reasonably precise estimates of the overall enlistment responses to the test programs, but not of the responses among subgroups of the population. Blacks, for example, account for only about 10 percent of high-quality male enlistees. The standard error of an estimated response among blacks would be approximately 10 percentage points, too large to make the estimate of practical value. For this reason, this study did not explore social representativeness issues.

THE EATP AS POLICY SIMULATION

Most of the discussion of test results in this and the following sections is couched in terms of "program effects." The experiment was intended to simulate the long-term effects of offering each of the test programs nationwide, but this was probably too ambitious a goal for

[38]See, for example, the testimony of Charles C. Moskos, Jr., before the House Committee on Veterans' Affairs; U.S. House of Representatives (1981), Vol. II, pp. 65-66.

this one-year test. For several reasons, discussed below, we might expect the steady-state responses to the various programs to differ from the responses during the test period. Thus the "program effects" that this study identifies should be interpreted as "effects of the program, as implemented and advertised, under experimental conditions."

The limitations of the test as policy simulation fall into two general categories: (1) those arising from the impossibility of completely isolating the treatment groups (test cell populations) and (2) problems associated with the limited period of the test. As a result of the former, some individuals may have left their home areas to enlist in areas where more generous benefits were being offered, while others may have been influenced in their decisions to enlist by advertising of benefits that, unknown to them, were not available in their home areas. The limited period of the test meant that awareness of the programs may not have been as great as it would be in the long term, that advertising had to be used in an attempt to correct this, and that the timing of some enlistment decisions may have been affected by the test.

Isolation of Test Cells

We cannot rule out the possibility of some cross-cell movements by individuals to take advantage of more generous benefits, but there are reasons to think such movements were not common. First, no advertising of the programs appeared in national media; all ads were placed in local radio and print media, preventing substantial spillover. Potential recruits were also not likely to learn of other areas' benefits through their local recruiters, who had a strong incentive not to volunteer information about benefits elsewhere (each recruiter is rewarded only for those recruits he brings in directly). Finally, from the Applicant Survey (Orvis and Hawes (forthcoming)) comes direct evidence on migration. Of all individuals who took the first step toward enlistment in April 1981—taking the military aptitude test—1.3 percent reported taking the test outside their local areas in the knowledge that educational benefits were different there, and only 0.2 percent admitted moving to take advantage of better benefits. Thus, it appears that cross-cell migration probably was not important.[39]

[39]Haggstron, et al. (1981) attempted a straightforward test of actual migration as indicated by data in the individual's enlistment record. They were limited by the available data, however, to examining differences between the AFEES of an individual's enlistment and of his home of record. They noted: "We found no compelling evidence that deliberate 'recruiting migration' had taken place, and the number of migrants was too small in any case to affect our estimates appreciably."

Limited Test Period

The short one-year period of the test presents more serious obstacles to interpreting the test results as simulations of the effects of nationwide policy changes. Because of this limited period, it may not be possible to observe steady-state effects. Enlistments in a test program cell may have been inflated by individuals who would otherwise have enlisted before or after the test, had the program been available then, or held down by a lack of awareness, among potential recruits and those who influence their decisions, of the availability of the programs.

Intertemporal Enlistment Shifts. It is convenient to think of the pool of potential recruits in one year as independent from that in the next year, as if each pool consisted of a single-year age cohort. In fact, enlistees enter at various ages; most at 18, but substantial numbers at 20 and older. Thus any new program, the EATP test programs included, may attract some youths who had previously rejected military service. At the other end of the test period, the test programs may have hastened the decisions to enlist of some individuals who would otherwise have enlisted after the end of the test. This need not have been because they knew that the programs were about to be discontinued; it could simply be that stronger enlistment propensities generated by the programs led them to enlist earlier.

Testing for intertemporal effects is, unfortunately, impractical. Although such effects should alter the age distribution of enlistees, other factors could equally well explain any observed changes; for example, educational benefits may appeal most strongly to individuals a little younger, or older, than the typical enlistee.[40] We should not expect large intertemporal effects, however, because most recruits wait only a short time after their first recruiter contact—on average, less than two months—before enlisting.

Awareness of Programs. Probably the most serious problem for interpreting the test results as long-term program effects is the difficulty of achieving the same level of awareness of the programs among the civilian populace as there would be for an established program. Recruiters attempt to contact every new high school graduate, and could be expected to give information about the test programs to many of them, but of course not every graduate is actually contacted. Two issues are involved: (1) awareness may not have been as high as under a permanent program and (2) the test programs were extensively advertised, and this advertising may have affected enlistments directly as well as through its effect on awareness of the programs.

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[40]We might also expect to find enlistments bunched at the beginning and end of the test period. The evidence on this point is inconclusive: see Sec. V and App. C.

There is no way to determine the importance of the problem of possibly lower awareness of the test programs. Although surveys tell us about awareness among potential recruits during the test period,[41] comparable data are not available for the GI Bill, an appropriate comparison program, nor do we have any way to determine whether high school counselors, parents, and others who influence the career decisions of young people were aware of the EATP programs. It seems likely that this group of "influencers," even more so than the group of potential recruits, was not as aware of the availability of generous educational benefit programs during the test period as it was during the earlier GI Bill years. If awareness was indeed lower, then the "program effects" presented in Sec. V understate the responses that the test programs would generate in the long run if they were implemented nationally.

The advertising expenditures reported in Table 6 were a direct attempt to overcome the awareness problem. However, because these expenditures were net additions to what otherwise would have been spent in the test program areas, they may also have generated enlistments simply by virtue of subjecting each potential recruit to a greater number of advertisements.[42] In assessing the effects of the test programs, we would like to be able to take out this advertising effect. Unfortunately, the distinction between the informational and sales contents of advertising is more useful as a conceptual than a practical tool. Lacking any way to separate the two effects, we must be cautious in interpreting the estimated program effects in the test, remembering that they may include some portion that should be attributed to the increased advertising.

Limited awareness of the test programs may have held the observable program effects below what they would be for permanent programs. Advertising may have inflated the program effects. The test data do not permit us to determine which of these two offsetting effects was stronger.

Conclusions

It is tempting, but inappropriate, to assume that if one of the EATP test programs raised enlistments by 10 percent during the test, it will

[41]Orvis and Hawes (forthcoming) report data on media recall and general awareness of the EATP programs, among individuals who took the military aptitude test.

[42]The alternative—diverting dollars from other advertising messages—would not have been an improvement. Awareness of other service programs, such as job training, would then presumably have fallen.

do the same if it is made a permanent part of the services' enlistment incentive packages. With the same program available everywhere, there could be no question of migration, either of individuals or of information. A permanent program might induce an initial enlistment surge, but the response would then settle into its steady-state rate. Finally, a permanent program would gradually become known (even without extensive advertising) among high school counselors, college financial aid officers, and—in part through these groups—to the young people who might consider entering the military to receive support for their further education. The Educational Assistance Test Program could not completely simulate the effects of permanent programs because in each of these areas—migration, intertemporal effects, and awareness—the test results may differ from steady-state responses.

Despite the limitations of the test, we believe the experimental results indicate the general types and approximate magnitudes of effects that could be expected of permanent programs. Probably the most important caveats are that the availability of the test programs may not have been generally known and, perhaps offsetting the first, that some direct advertising effects may be included in the program effects presented in Sec. V.

IV. DATA AND METHODOLOGY

The methodology used to analyze the Educational Assistance Test Program imposes some limitations on the sorts of data that may be brought to bear, but it also eliminates the need for certain variables that are either not available or whose effects are difficult to measure. Therefore, this section first discusses the methodology, then describes the data collected for the analysis. Following some summary measures of the data that provide a first look at the effects of the test programs, the section concludes with a more complete description of the statistical methodology.

BASIC METHODOLOGY

Our methodology consists, at its simplest level, of comparing each of the test program cells with the control cell in terms of their growth (or decline) in enlistments between a base period (December 1979-September 1980) and the test period (December 1980-September 1981). Each cell thus serves as its own control for structural differences across areas of the country in such factors as long-term employment conditions, demographic characteristics (age distribution, race), urban/rural mix, etc. Taken together, these factors determine the basic propensity to enlist. Surveys have attempted to measure this propensity, but their precision is not well established and their sample sizes have been too small to provide useful geographic detail. Rather than attempting to account for all the factors—some measurable, others not—that determine the propensity to enlist, we measure it in an indirect but straightforward way: through the base-period enlistment level. It might be desirable to use a longer base period than ten months, but we would then have to tread on the period of the Multiple Option Recruiting Experiment.[1] A longer base period would also increase the probability that some of the structural factors that we assume do not change have indeed changed.

Simply comparing test period enlistments in a particular program cell with past performance is not sufficient. Many things changed be-

[1]Even the chosen base period is not entirely free from the influence of MORE; the Navy test continued until the end of March. In the analysis presented in Sec. V we account for the influence of the MORE test programs on Navy enlistments. As noted in Sec. III, however, none of the MORE options had sizable effects on Navy enlistments during the ten months of the test examined by Haggstrom et al.

tween 1980 and 1981 that might have affected enlistments nationwide. As with the structural differences across areas, it is difficult to measure the changed factors, or to measure their effects, or even to know what factors should be measured. Who can assess, for example, the effect of the hostage situation in Iran on military enlistments in 1980 and later? The Army experienced a 32.3 percent gain in enlistments in the UVK cell in the test period compared with the base period, but how much of this gain is attributable to the introduction of the new program, and how much to a generally more favorable recruiting environment?

The answers to these questions are provided by the control cell. In the control cell, none of the services changed their educational benefit offerings between the base and test periods. We may not know what factors other than the test programs have affected enlistments, or what their individual effects have been, but the control cell tells us what effect, in the aggregate, they have had on enlistments. The enlistment gain experienced by the control cell is the gain that we should have expected in the three other cells had there been no EATP. By comparing the enlistment gain in a test cell with the gain in the control cell, we can derive an estimate of the test program effect. Thus if we find that Army enlistments in the control cell were up 21.7 percent (as indeed they were), we can conclude that the UVK program increased enlistments by 8.7 percent (1.323 divided by 1.217).[2]

Comparisons with the control cell eliminate the effects of changes that have affected enlistments nationwide, but they do not eliminate changes—such as a sudden slump in new car sales that throws thousands out of work in Detroit—that affect enlistments in only limited areas of the country. We can, with some justification, safely ignore such changes, for three reasons. First, they are not likely to be so localized that they will affect enlistments in only one cell: each cell has an AFEES in every major area of the country (see App. B), and even an industry such as automobile production that is traditionally associated with a single city is in reality much more geographically diverse in its employment. Second, very local influences will affect only a portion of any one test cell because each cell is geographically dispersed, and so will not have a major impact on total cell enlist-

[2]This method, simple though it is, proved to be remarkably reliable for estimating the program effects, and it had the tremendous advantage over the more complicated statistical procedures described below that it could provide estimates long before the data required for the more complete analysis could be obtained. Rand reported results for the first five months of the test (December-April) in early June 1981. Although we could not place a great deal of confidence in these early estimates because their standard errors were large, and because the early responses may have reflected start-up lags, the final results reported in Sec. V are only insignificantly different.

ments. Finally, the processes that generate changes will be independent from the process that generated the test cells; there were not riots in Los Angeles, for example, simply because that city had been relegated to the control cell. Ignoring changes does not give us wrong results, though it may not yield the best estimates possible of the programs' true effects. (Technically, our estimates would be unbiased, but perhaps inefficient.)

To improve our estimates of the programs' effects, we control in a multivariate regression model for such changes as we can measure. To capture their effects, however, we must discard the simple four-cell division of the country and ten-month aggregate measure of enlistments in favor of a finer division. A natural geographic division of the country is into individual AFEES areas, and of time into months. An even finer division would provide more observations if reliable measures of the relevant data were available more finely divided, but they are not. At this level of disaggregation, there are various measures of civilian labor market conditions published by the Bureau of Labor Statistics and data on recruiter force levels provided by the services. Better data on labor market conditions and on other variables such as family incomes or federal government expenditures might be available if we were content with only single annual observations and content to wait another year before completing this analysis, but it is doubtful that we would be able even then to accurately measure these variables' effects.

DATA

The data collected for this study fall into four categories: (1) enlistments, (2) youth populations, (3) civilian labor market conditions, and (4) the services' recruiting efforts. The Defense Manpower Data Center (DMDC) provided records of individual enlistment contracts during fiscal year 1981, from which we generated counts of enlistments, by AFEES and month, in various categories. DMDC produced similar counts for fiscal year 1980. A special data tape prepared by the Census Bureau for the use of the National Cancer Institute yielded estimates of youth populations by county. DMDC provided information on the correspondence between county areas, and the areas of AFEES and of the individual services' recruiting districts. Four series measuring civilian labor market conditions were taken from the Bureau of Labor Statistics' publication *Employment and Earnings*, various issues. Finally, each of the services provided data on the numbers of

their production recruiters.[3] Each of the data series is described below, and their usefulness assessed. For reasons that are discussed in the final portion of this section, all variables that enter the regression model do so as the logarithm of the ratio of the observation for each month in the test period to the observation for the same month of the base period.

Enlistments

The enlistment records provided by DMDC were of individual non-prior service enlistment contracts. Most recruits enter a delayed entry program (DEP), resulting in a lag between their contract and active duty dates. Eligibility for the EATP programs was determined by the date of the individual's enlistment contract, which led to our use of data on contracts rather than on accessions. It should be noted, however, that some individuals do drop out of DEP, so our data will overstate the official numbers of enlistments (accessions) reported by the services. In addition, our data, which come from the AFEES Reporting System (a function of the Military Enlistment Processing Command—MEPCOM), do not agree precisely with the services' counts of contracts derived from their computer reservation systems. It is not clear which set of data is more reliable, but in any event the differences among the data from different sources do not appear to vary systematically across areas of the country.[4]

The DMDC records provide a variety of information about each contract, of which seven elements were selected to classify the recruits into approximately 80,000 cells:[5]

1. Service: Army, Navy, Marine Corps, or Air Force.[6]
2. AFEES: a total of 69, including two substations (Anchorage and San Diego).
3. Month of contract: October, November, . . ., September.

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[3]"Production recruiter" is not consistently defined across the services, but generally refers to recruiters who have been assigned enlistment quotas.

[4]Month-to-month differences between the DMDC data and the services' contract data are significant, because the services' data are based on recruiting months, which are arbitrarily defined to include integral numbers of recruiting weeks (Tuesday to Monday).

[5]Many of these cells were empty: there were fewer than 400,000 enlistments in FY81. Most of our analysis was conducted on aggregates of many cells; all AFEES in the test cell, for example, or all ten months of the test.

[6]Included in the file were records for approximately 13,000 Navy reservists (FY81) serving extended active duty tours, of whom less than one-fourth were high-quality males. These reservists were excluded from the Navy counts.

4. Educational attainment: high school diploma graduate or nongraduate.
5. Mental aptitude: AFQT score at or above the 50th percentile (mental categories I-IIIA) or below the 50th percentile (IIIB-IV).
6. Sex: male or female.
7. Specialty enlisted for: Army—MOS in group I, group II, or other; Air Force—eligible AFSC or other; Navy, Marine Corps—specialty information not available.

The classification of educational attainment needs further explanation. Many recruits sign enlistment contracts while they are seniors in high school, planning to begin active duty after graduation. Such enlistees were eligible for EATP benefits, provided that they did in fact graduate. Accordingly, we have included these seniors, who are identified on the DMDC records with a special code, as high school graduates.[7]

To check the accuracy of the specialty coding in the DMDC records, we obtained counts of high-quality male enlistments in the EATP-eligible specialties from the Army and Air Force, and found that they corresponded quite well with our counts. It appears that future researchers can use with some confidence the specialty information on the DMDC contract records for these two services. The records contained only incomplete information on the specific ratings in which the Navy promised training, preventing us from analyzing skill channelling in the Navy.

Populations

Population data did not enter directly into the analysis. AFEES populations fall into the category of long-term demographic differences, which were implicitly controlled for by our methodology. Several of the variables described below, however, were not available for AFEES areas, requiring the use of county population estimates for constructing AFEES observations as population-weighted averages of observations for the states, district recruiting commands, etc., served by each AFEES. The population estimates were for males aged 15 to 19 in 1978. These served as estimates of the 17 to 21 year old population, the prime enlistment age, in 1980. Differences across areas in the year-to-year change of this population are small enough to be ignored.

[7]This code was first used in fiscal year 1979. Before that year, seniors were identified only as nongraduates with 11 years of education. This is one reason we did not extend our base period farther into the past.

Civilian Labor Market Conditions

Four variables that measure employment opportunities in the civilian economy proved to be important in explaining differences across areas in enlistment gains: (1) average hourly earnings of production workers on manufacturing payrolls; (2) average weekly hours for the same group; (3) total nonagricultural employment; and (4) the unemployment rate for all workers 16 years and older. These variables appear to be quite closely related—all respond to short-term charges in overall business conditions—so it is perhaps surprising that all four enter the model significantly. They do differ in certain respects, however, in how they respond to changing business conditions and in their potential effects on the enlistment decisions of individuals. In addition, although taken together the four variables probably serve as a good proxy for the employment prospects of youths, individually they may be correlated with different components of the variation in the unobservable "true" variable (or variables) that accurately measures these prospects.

The close relationships among the variables can be seen by considering what happens when there is a general increase in demand for goods and services. Businesses first respond by asking (or requiring) their production employees to work longer hours. If this involves more overtime pay, average hourly wages will also rise. Businesses next hire more employees, which requires, if the labor market is at all competitive, the paying of higher wages. More workers employed mean fewer unemployed, but there may be a partially offsetting movement of people into the labor force who previously had not been actively looking for work. In a downturn the directions of each these changes is reversed, although the last movement may be slower.

That movements in the four variables result from the same causes does not mean that they are highly correlated, nor does it mean that they will have the same effect on military enlistments. As the discussion above indicates, the four do not move in lockstep; changes in hours generally precede changes in employment, for example.[8] Depending on the precise nature and causes of the particular upturn or downturn, movements in the variables will also differ in degree. Finally, each of the variables can have an independent effect on enlistments. Wages and hours together determine workers' take-home pay, and hence the willingness of young workers (or those seeking work) to enlist at given military pay levels. Increases in hours also

[8]"Average weekly hours in manufacturing" is designated as one of 12 leading economic indicators by the National Bureau of Economic Research. Rones (1981) presents recent evidence on the relative timing of hours and employment changes.

signal a shift of some workers, particularly youths and other secondary workers, from part-time to full-time status. Employment increases mean that more jobs are available, which should reduce enlistments, but if the employment gains are accompanied by large movements into the labor force there may be little reduction in the unemployment rate; because youths make up a large proportion of the unemployed, the net effect may be only a small increase in their chances of finding civilian jobs. In short, omission of one or more of these variables can be expected to reduce significantly the ability of the model to explain changes in enlistments.

All the civilian labor market variables share the apparent defect that they are not specific to the group of enlistment-age young men and women. The selection of these variables was driven, of course, by necessity. No geographic detail on age-specific unemployment is available because the sample size of the Current Population Survey, from which unemployment data are derived, is too small to permit it. The Bureau of Labor Statistics' *Establishment Survey*, which yields employment, hours, and wages data, is large enough to permit fine disaggregation, but asks no questions about workers' ages.

Fortunately, the defect of these variables is more apparent than it is real. Consider, for example, the unemployment rate and employment data. It would seem that a youth unemployment rate would be a better predictor of enlistments: teenagers (16 to 19 years old) make up only about 9 percent of the civilian labor force, and the swings in their unemployment rate are less severe than are those of the aggregate unemployment rate.[9] These statistics, however, are very deceiving. Teenagers account for one-fourth of the unemployed,[10] and although their unemployment rate is less cyclically sensitive than are those of other groups, this is largely because the labor force participation rate of teenagers is very sensitive to cyclical fluctuations. Indeed, more than one-fourth of cyclical swings in total employment consist of changes in the employment of teenagers; this does not show up as swings in teen unemployment because 35 percent of any cyclical gain in teen employment is eaten up by the movement of teens into the labor force.[11]

The importance of these points for the current study is that a youth unemployment rate, were it available on a geographically disaggregated basis, might well prove to be a poorer predictor of military en-

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[9]Between 1972 and 1975, for example, aggregate unemployment rose from 4.8 to 8.5 percent, whereas teen unemployment went from 14.5 to 19.9 percent, falling from 3 times the aggregate rate to only 2.4 times.

[10]Over the past ten years this proportion was highest in 1973 (28.5 percent) and lowest in 1981 (21.4 percent).

[11]This discussion is based on work by Clark and Summers (1981).

listments than are the two series on general employment and unemployment. Youths make up a small fraction of the *level* of the employment variable, but a large fraction of its changes. Were we trying to explain differences across areas in the levels of enlistments or of enlistment rates (enlistments divided by population), youth-specific employment data probably would be superior, but the large swings in youth labor force participation rates make the youth unemployment rate a poor measure of *changes* in the employment prospects of enlistment-age young men and women.

The large cyclical swings in youth employment also make it likely that any series on youth wages or earnings that is available, even at the national level, will be a poor predictor of the earnings prospects of any individual youth. Average wages or earnings are, after all, the averages for those who are employed. If the youths who are forced out of work by a recession had been low paid, we could observe the perverse result, in a youth earnings series, of average earnings rising despite a fall in the earnings of any individual youth. This could occur, of course, for a series on the earnings of all workers, but the large cyclical swings in youth employment make it particularly likely for this group. Our selection of the average wage in a single industry—manufacturing—further minimizes the problem of perverse movements by eliminating that component attributable to shifts in the industrial composition of employment. Thus, although young workers make up a very small proportion of manufacturing employment, average wages in this industry, which feels particularly strongly any cyclical changes in total demand for goods and services, may provide a very good measure of the changes in the earnings prospects of youths that are presumed to affect their enlistment decisions. Of course, if these decisions are determined more by long-run changes in civilian wage levels than by short-run swings, then this variable will not be a good proxy for the relevant youth wage measure, but if it is not this will be evident in an insignificant regression coefficient.

Recruiting Effort

Recruiting effort was measured in this study by the numbers of production recruiters fielded by each of the services. It is natural to assume that more recruiters in an area means more high-quality recruits, but we should not expect the relationship to be proportional. That is, adding one recruiter to an area will increase enlistments by less than the average production of his predecessors. It should also be noted that the parameter estimate derived for this variable will not be a measure of the true marginal productivity of recruiters in high-

quality recruits, but only their marginal productivity given that they must also bring in some lower-quality recruits to meet their quotas. The variable will also capture the effects of changes in the numbers of recruiters' aides and in advertising expenditures, to the extent that these resources are allocated across areas in proportion to the numbers of recruiters. We were unable to obtain direct data on these other components of recruiting effort.

The recruiter data were reported by quarter; linear interpolation turned these into monthly estimates. The necessity for interpolation introduced some measurement error into the recruiter variables, but the error should not cause severe problems because the quarter-to-quarter variation within individual AFEES was small in comparison with the differences across areas in year-to-year changes.

The form of our model should give us much more reliable estimates of recruiter effectiveness than are available in previous studies. Two problems have affected past studies, although their potential importance appears to have gone unrecognized.[12] First, when a recruiter variable is introduced in a cross-section model, it captures not only the effects of recruiters but also those of the demographic variables that were used by the recruiting command, either explicitly or implicitly, to assign recruiters to areas. Failing to account for this problem has inflated the recruiter coefficient estimates of past studies. The current model solves the problem because it looks at year-to-year *changes* in recruiter levels, which can hardly have occurred in response to similarly changing demographics. Second, recruiters may be moved between areas in response to past recruiting performance; into areas of good performance, for example, to take maximum advantage of the situation.[13] This will particularly affect models that pool time-series and cross-section data, introducing a correlation between the recruiter variable and the error term in the equation. This will also bias, probably upward, the estimate of recruiter effectiveness. With the current model, however, the recruiting command's response to past performance would have to be very fast indeed to substantially bias our estimate. An accurate estimate of the effectiveness of recruiters will turn out to be very important because, as shown below, the four cells in the experiment did not experience similar year-to-year changes in recruiter force levels.

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[12]Our review of the literature on enlisted supply found no studies that discussed either problem.

[13]Current Army practice limits this possibility. The geographic areas assigned to recruiters are changed only every three years.

Converting to AFEES

Of the data described above that enter the regression model as explanatory variables, only one series—Air Force recruiters—was directly available for AFEES areas. The Navy and Army could report their counts of recruiters only for their recruiting districts (43 in the Navy, 56 in the Army), and the Bureau of Labor Statistics (BLS) reports data for states.[14] We were forced, therefore, to estimate the values of each variable for a particular AFEES by weighting the values for the states (or recruiting districts) served by that AFEES. At the request of the Army Recruiting Command, this conversion was done for Army recruiters by DMDC; DMDC's procedure was conceptually similar to ours, which is described below.

The five data series (Army recruiters and the four labor market variables) fall naturally into two groups: conditions (hourly earnings, weekly hours, and unemployment rate), and numbers of people (employment, recruiters). The conditions we assume apply to each individual youth within a state, regardless of where he lives. The numbers-of-people we assume are spread across the state (recruiting district) in proportion to the youth population. With data on youth populations by county (described above), and information on the county composition of each state, recruiting district, and AFEES area, these assumptions yield the required AFEES estimtes. For the condition variables, the weights are the proportions of the AFEES population residing in each state; for the number of people variables, the AFEES estimate is formed as the weighted sum of the levels in each state (recruiting district), the weights being the proportions of the states' populations served by the particular AFEES.

SUMMARY MEASURES OF THE DATA

Enlistments

Table 7 gives counts of total high-quality male nonprior service enlistments, by test cell and nationwide, for each of the services examined in this study, in both the base period (December 1979 to September 1980) and the test period (December 1980 to September 1981). It also provides initial estimates of the program's effects. The column headed "Ratio Test to Base" gives the ratio of test period to base period enlistments for each cell/service, and the column "Rela-

[14]BLS also reports data for selected metropolitan areas, but we made no attempt to exploit this less reliable detail.

Table 7

HIGH-QUALITY MALE NONPRIOR SERVICE ENLISTMENTS BY SERVICE AND CELL

Service/ Test Cell	Enlistment Contracts		Ratio Test to Base	Relative Increase (Percent)
	Base Period	Test Period		
Army				
Control	12041	14650	1.2167	—
UVK	4189	5541	1.3228	8.72
NCV	3749	4556	1.2153	-0.12
T/S	3750	4284	1.1424	-6.10
Total	23729	29031	1.2234	—
Navy				
Control	14056	13393	0.9528	—
UVK	5560	5742	1.0327	8.39
NCV	4742	4673	0.9854	3.42
T/S	4559	4701	1.0311	8.22
Total	28917	28509	0.9859	—
Air Force				
Control	16982	18155	1.0691	—
UVK	6251	6778	1.0843	1.42
NCV	5095	5643	1.1076	3.60
T/S	5110	5766	1.1284	5.55
Total	33438	36342	1.0868	—

tive Increase" gives the ratio of the absolute increase in each of the test program cells to the increase in the control cell, expressed in percentage terms. As indicated above ("Basic Methodology"), these relative increases may be presumed to be unbiased estimates of the effects of the test programs on high-quality male enlistments.

Nationwide, the Army showed the largest gain in enlistments, more than 22 percent, whereas the Air Force registered a small gain and the Navy a small loss. The largest gains for the Army and Navy appeared in the UVK cell, and for the Air Force in the T/S cell. In terms of increases relative to the control cell, it appears that UVK was the most successful of the programs, raising Army enlistments by 9 percent and apparently causing a spillover that raised Navy enlistments as well. The no-contribution provision embodied in the NCV program

seems to have had little effect. Results for the T/S cell appear at first glance to be contradictory: it raised Navy and Air Force enlistments but reduced Army enlistments, for a net gain over the three services of 2.5 percent. These results are consistent, however, with the hypothesis that eliminating the advantage given the Army by its kickers in the control program would hurt Army recruiting.

Explanatory Variables

Table 8 shows the ten-month averages of the civilan labor market variables, by test cell and nationwide, for the base and test periods, and the percentage change of the averages between the two periods. Corresponding data for the three services' recruiter variables appear

Table 8

CIVILIAN LABOR MARKET VARIABLES: AVERAGES BY CELL

	Base Period	Test Period	Percent Increase
Hourly earnings			
Control	$ 7.30	$ 8.05	10.4
UVK	7.12	7.85	10.2
NCV	7.38	8.18	10.9
T/S	7.24	7.98	10.3
Nation	7.27	8.02	10.4
Weekly hours			
Control	39.7	40.0	0.8
UVK	39.8	40.0	0.4
NCV	39.8	40.3	1.2
T/S	39.5	40.1	1.6
Nation	39.7	40.1	0.9
Employment (000s)			
Control	49915	50179	0.5
UVK	18160	18306	0.8
NCV	13712	13812	0.7
T/S	14550	14654	0.7
Nation	96337	96951	0.6
Unemployment rate			
Control	6.97%	7.41%	6.3
UVK	6.74	6.97	3.4
NCV	7.76	8.09	4.2
T/S	6.74	7.20	6.8
Nation	7.01	7.40	5.5

in Table 9. For the three "condition" variables—earnings, hours, and unemployment—the averages reported are weighted means of the ten-month averages within each AFEES, the weights being the 17 to 21 year old male population of the AFEES. Because of this weighting scheme, the nationwide averages will not agree exactly with officially reported data, but the numbers in the tables will give a more accurate impression of the enlistment effects of the variables than if other weights had been used.

Table 8 shows that the cells were not as well balanced as might have been hoped. The Noncontributory VEAP cell, in particular, had generally higher unemployment, and slightly higher wages, than the other cells. In large part this was due to the influence of the Detroit AFEES, one of those in the NCV cell, where the unemployment rate has been well above 10 percent since January 1980.

Differences across cells in the levels of the variables are less important for the analysis than are differences in the year-to-year changes. The greatest differences in these changes are for the unemployment and recruiter variables. Most striking are the declines in Navy and Air Force recruiter force levels in the Tuition/Stipend cell. Controlling

Table 9

PRODUCTION RECRUITERS BY SERVICE AND CELL

	Base Period	Test Period	Percent Increase
Army			
Control	2529	2501	-1.1
UVK	884	895	1.2
NCV	757	750	-0.9
T/S	744	763	2.6
Nation	4914	4909	-0.1
Navy			
Control	1714	1746	1.9
UVK	675	696	3.2
NCV	501	529	5.5
T/S	595	566	-4.9
Nation	3485	3538	1.5
Air Force			
Control	917	977	6.5
UVK	329	336	2.1
NCV	285	304	6.5
T/S	328	314	-4.2
Nation	1858	1930	3.9

for these declines should raise the estimates of the effects of this program on Navy and Air Force enlistments. Of course, the extent to which the evident differences affect the program effect estimates depends upon how great is the influence of the variables on enlistments, something that is explored in Sec. V.

THE REGRESSION MODEL

Although Table 7 provides unbiased estimates of the test programs' enlistment effects, the estimates can be improved by controlling for changes in the general recruiting environment caused by factors other than the test programs that may have differed across cells. Tables 8 and 9 showed that the cells did not experience identical changes in unemployment, recruiter force levels, etc. Controlling for these changes will, therefore, alter the program effect estimates. Even were the cells perfectly balanced in terms of the changes in our explanatory variables, however, there would still be an efficiency gain to be realized from entering the variables in a regression model so long as the changes were not identical in all AFEES areas and so long as the variables do, indeed, influence enlistments. The regression model also allows us to control for the effects of two small-scale experiments conducted by the Army and Navy during the 1980 base period, and to explore possible trends in the responses to the programs.

Two assumptions underlie the specification of our regression model:

1. The number of enlistments in any AFEES/month is generated by a nonhomogeneous (time-varying) Poisson process.
2. The function relating the expected enlistment counts to the determining variables is multiplicative in its arguments. That is, the effect of a variable (or test program) is proportional to the base number of enlistments.

Haggstrom et al. (1981) have previously given sufficient conditions for assumption (1) to hold, and derived its implication for the current problem. Here we summarize the basic points; the interested reader is referred to the earlier work for details.

Let λ_{it} denote the expected enlistment count in AFEES i during month t. Given assumption (2), we have

$$\log(\lambda_{it}) = \alpha_i + \beta_t + \gamma_k + \theta_{it} + \delta' X_{it} , \qquad (1)$$

where the α_i are effects specific to individual AFEES but constant over time, the β_t to particular months but constant across AFEES,

the γ_k the effects of the four programs (test plus control), the θ_{it} are AFEES-specific seasonal effects, the X_{it} are vectors of the logarithms of values of the "explanatory" variables, and δ is a vector of parameters reflecting the effects of those variables.

Equation (1) could be estimated directly by linear regression. Let y_{it} denote actual enlistments in AFEES i during month t. Then using a result of Cox (1955), we substitute as the dependent variable $\log(Y_{it}) = \log(y_{it} + 1/2)$, which has mean approximately equal to $\log(\lambda_{it})$ and variance approximately $1/\lambda_{it}$. The resulting equation, however, would have a large number of effects to be estimated. We simplify by assuming that θ_{it} are unchanged from year to year (i.e., $\theta_{it} = \theta_{i,t-12}$), and taking as the dependent variable the difference in $\log(Y_{it})$ for the same month in consecutive years. Noting that γ_c, the "treatment" effect in the control cell, is the same effect as in the entire country during the previous year, we have

$$\log(Y_{it}) - \log(Y_{i,t-12}) = (\beta_t - \beta_{t-12}) + (\gamma_k - \gamma_c) + \delta'(X_{it} - X_{i,t-12}) + (\epsilon_{it} - \epsilon_{i,t-12}), \quad (2)$$

where the ϵ_{it} are the error terms. If the model specification is correct, the error terms in Eq. (2) are independent with means near zero and approximate variances

$$\sigma^2_{it} = 1/\lambda_{it} + 1/\lambda_{i,t-12}.$$

In the presence of unequal variances across observations (heteroskedasticity), estimation of Eq. (2) by ordinary least squares would be inefficient, and would yield biased estimates of the standard deviations of the parameters. The parameters may be estimated, however, by weighted least-squares linear regression with weights $w_{it} = 1/\sigma^2_{it}$. Because the λ_{it} are unknown, the variances must be estimated from the data. A straightforward method is to estimate λ_{it} by y_{it}, yielding

$$\hat{\sigma}^2_{it} = 1/y_{it} + 1/y_{i,t-12}.$$

An analyst who did not explicitly consider the process generating the enlistment counts might well have been led to estimate an equation similar to (2). He certainly would have realized that the variance of enlistments differs substantially across AFEES areas, and would have had to face the problem of what weights to use in his regression. He might have been led to use populations in his weighting instead of enlistments, but this would not have led to substantially different results, affecting only the efficiency, but not the bias, of his parameter

estimates. He would not, of course, have added the "1/2" correction factor to his enlistment counts, but this is not important unless the counts are generally small—less than 20, say. In short, while the Poisson assumption is useful, as a practical matter it is not a vital part of the regression model, serving principally to give a little more confidence in the model's applicability.[15]

To be estimated, Eq. (2) requires only the addition of indicator variables for the three test programs (UVK, NCV, and T/S) and the ten months. Two minor modifications, however, give some of the parameter estimates more useful interpretations. First, we eliminate one of the month indicator variables (December), replacing it with a constant term and give the other month indicators the value 0.9 if the observation falls in the particular month, and −0.1 otherwise. This gives the estimate of the constant term the interpretation of the average gain in enlistments, over all ten months, in the control cell. The coefficients for the month indicator variables indicate by how much the enlistment gains in those months deviated from the gain in December (the excluded month), controlling of course for the effects of the test programs and of the other variables. Second, we measure each of the explanatory variables as deviations from their means over all i and t. This gives the constant term the interpretation of what would have been the average gain in control cell enlistments had that cell experienced the nationwide average change in each of the variables such as unemployment, recruiters, etc. This modification does not affect the estimated effects of the "X" variables (δ).

[15]The Poisson model does give the tremendous advantage, so important in the course of this study, that it permits the calculation of standard errors of program effects estimated from simple comparisons of cell enlistment totals, such as the estimates that appear in Table 7. The analyst considered in the paragraph above, who probably would have assumed that his enlistment counts were approximately normally distributed (or perhaps log-normally), would have had to complete his regression analysis before deciding whether the apparent effects of the program were indeed statistically significant.

V. TEST RESULTS: HIGH-QUALITY MALE ENLISTMENTS

This section presents the results from estimation of the regression model, described in the previous section, for high-quality male enlistments in each of the three services: Army, Navy, and Air Force. Preliminary analysis indicated that little would be gained from estimating the model for other groups of enlistees (e.g., females, lower-quality males), as these groups showed little or no response to the test programs.

Table 10 gives the regression results. A total of 660 observations were available, 66 AFEES for each of ten months. In addition to the variables described in the previous section, an indicator variable was included in the Army regression to measure the effect of the two-year option tested during fiscal year 1980, and three variables in the Navy regression to capture the effects of MORE test packages that were offered through February 1980.

The overall fit of the model is quite good in each of the services. Although the squared multiple correlation coefficients (R-squared) appear low, suggesting that the included variables explain only a small part of the variance in the change in enlistments, this appearance is misleading. Under the Poisson model, much of the variance in enlistment counts is attributable to the fundamental randomness of individual enlistment decisions. Of the variance not attributable to this randomness, the included variables explain about half; least in the Army and most in the Navy. For a model that is attempting to explain *differences* across areas in *changes* over time, each of which is notoriously hard to explain alone, this performance is very good.

EFFECTS OF THE TEST PROGRAMS

The test program estimates in Table 10 are not immediately useful because they indicate the changes in the *logarithm* of enlistments associated with each of the programs. Table 11 converts these estimates into percentages, and again indicates those estimates that differ significantly from zero at the 5 percent confidence level. The control cell is the omitted cell in the regressions, so its enlistment gain appears in the constant and all the program effects are measured relative to it.

Table 10

Regression Results for High-Quality Male Enlistments

	Army		Navy		Air Force	
Constant	0.183[a]	(0.015)[b]	-0.040[a]	(0.014)	0.063[a]	(0.013)
Test programs						
Ultra-VEAP kicker	0.087[a]	(0.028)	0.093[a]	(0.027)	0.027	(0.024)
Noncontributory VEAP	0.013	(0.029)	0.038	(0.029)	0.054[a]	(0.026)
Tuition/Stipend	-0.059[a]	(0.030)	0.100[a]	(0.029)	0.075[a]	(0.027)
Changes in local labor market conditions						
Hourly earnings	-0.284	(0.663)	-1.392[a]	(0.654)	-1.442[a]	(0.581)
Weekly hours	-1.010[a]	(0.417)	-0.601	(0.442)	-0.816[a]	(0.379)
Employment	-0.532	(0.435)	-1.161[a]	(0.431)	-0.833[a]	(0.381)
Unemployment rate	0.255[a]	(0.092)	0.240[a]	(0.090)	0.286[a]	(0.082)
Changes in numbers of recruiters	0.295[a]	(0.115)	0.274[a]	(0.097)	0.090	(0.065)
Time trend						
January	-0.177[a]	(0.048)	-0.231[a]	(0.046)	-0.193[a]	(0.043)
February	-0.225[a]	(0.048)	-0.192[a]	(0.046)	-0.317[a]	(0.042)
March	-0.191[a]	(0.048)	-0.135[a]	(0.047)	-0.161[a]	(0.043)
April	-0.186[a]	(0.051)	-0.134[a]	(0.051)	-0.127[a]	(0.045)
May	-0.280[a]	(0.053)	-0.243[a]	(0.053)	-0.232[a]	(0.047)
June	-0.115[a]	(0.053)	-0.319[a]	(0.052)	-0.099[a]	(0.046)
July	-0.129[a]	(0.053)	-0.444[a]	(0.052)	-0.233[a]	(0.047)
August	-0.187[a]	(0.052)	-0.407[a]	(0.051)	-0.221[a]	(0.045)
September	-0.108[a]	(0.051)	-0.297[a]	(0.050)	-0.166[a]	(0.045)
MORE options						
Army two-year	0.032	(0.033)				
Navy A			0.014	(0.094)		
Navy C			0.072	(0.079)		
Navy D			-0.001	(0.067)		
R-squared	0.14		0.30		0.17	
F	5.65		13.95		7.90	

[a]Indicates t-ratio exceeding 1.96 in absolute value.

[b]Standard errors are in parentheses.

The regression results confirm the impressions gained from the simple cross-cell comparisons. The Army's program of $8,000 to $12,000 kickers raised Army enlistments by a statistically significant amount; about 9 percent. A larger gain might have been expected for this seemingly very generous program, but it must be remembered that it is being compared with an Army control program that included kickers of $2,000 to $6,000. In contrast with the test result, the Congressional Budget Office estimate, derived from a theoretical model based on present value comparisons with first-term pay (see Sec. III), is that nationwide implementation of the Ultra-VEAP kicker program would

Table 11

ENLISTMENT INCREASES DUE TO TEST PROGRAMS
(HIGH-QUALITY MALES)

Program	Army	Navy	Air Force
Ultra-VEAP kicker	9.1[a]	9.8[a]	2.7
Noncontributory VEAP	1.3	3.9	5.5[a]
Tuition/Stipend	-5.7[a]	10.5[a]	7.8[a]

[a]Based on regression coefficient that differs significantly from zero at 5 percent level.

increase Army high-quality enlistments by only 2 to 6 percent.[1] The Army's gain in the test did not appear to come at the expense of the other services, as shown by the relative increases recorded by both the Navy and Air Force.[2]

Results for the Noncontributory VEAP program were disappointing. None of the services registered a substantial relative gain in the NCV cell, and only for the Air Force can we be reasonably sure that the observed gain is not merely the result of random variation. It can be argued, of course, that the Navy and Air Force could not sell this program effectively because of the small numbers of specialties they had open to it, but this argument is belied by the more favorable showing of the Tuition/Stipend program in these two services, and certainly does not explain the particularly poor showing of the Army in the NCV cell.[3] In the Army, well over half of the enlisted specialties (in terms of job openings) were eligible for the NCV program. Moreover, the small gains registered by the NCV program in the Navy and Air Force could well be due, at least in part, to the additional $2700 in maximum *net* benefits it offers over the control program. For a provision that has been criticized as much as has the contribution requirement of VEAP, its removal seems to have done remarkably little.[4] The concluding section of this report discusses the important cost implications of this finding.

[1]Congressional Budget Office (1982), p. 37.

[2]The Marine Corps also showed an enlistment gain in this cell, relative to the control cell.

[3]The Army retained its control program kickers in the NCV cell, so an explanation for its poor showing does not lie there.

[4]Orvis and Hawes (forthcoming) report a similarly small effect of a contribution requirement. Eliminating such a requirement from a hypothetical $15,000 benefit program raised the estimated enlistment rate in their survey sample by only 2.1 percent.

Unlike the Noncontributory VEAP program, the Tuition/Stipend program gave substantial and statistically significant enlistment gains to both the Navy and Air Force. Indeed, the largest individual service effect for any of the test programs came from Navy enlistees in the Tuition/Stipend cell, despite a list of eligible specialties that included only about 10 percent of Navy training slots. This clearly casts doubt on the common assertion that Navy and Air Force recruiters could not effectively sell the test programs because so few enlistees could qualify for them. Perhaps recruiters were reticent about discussing details of the programs with potential recruits,[5] but apparently knowing that at least some of their recruits would qualify for a very generous program led Navy and Air Force recruiters to use educational benefits more aggressively in trying to sell young men on military service. Nonqualifying enlistees were still eligible, it must be remembered, for basic VEAP, which promised a nominal benefit of $8,100. The response to the Tuition/Stipend program might have been even larger in these two services had the program been more generally available, but the current results indicate that even a very narrowly targeted program, accompanied by a more modest basic entitlement, can elicit a sizable enlistment response. As was the case with the finding for the Noncontributory VEAP program, this has some important cost implications.

Probably the most unexpected of the test results were the Navy's large enlistment gain in the UVK cell and the Army's statistically significant loss in the Tuition/Stipend cell. The Navy had no special program in the UVK cell, and the Army, although it lost in the T/S cell the advantage given by its control program kickers, did have in T/S a much more generous program (see Tables 1 and 2). Both these results are evidence of substantial interservice effects.

The magnitude of the Navy UVK response—greater than in the Army—seems unreasonably large. As a check of this apparent effect, we examined UVK and control cell enlistments during the first nine months of fiscal year 1982, deriving a second set of estimates of the enlistment effects of the Ultra-VEAP kicker program (see App. D for a complete discussion of the data and comparisons).[6] For the Army and Air Force, these second estimates are virtually identical to those in Table 11, but the new estimate for the Navy indicates no spillover. Apparently, part of the large spillover suggested by the FY81/FY80

[5]Survey results in Orvis and Hawes (forthcoming) show that in the Tuition/Stipend and Noncontributory VEAP cells the details of the programs were but little understood even among those high-quality youths who actually enlisted.

[6]An Army policy change between fiscal years 1981 and 1982 clouded any interpretation of enlistment changes in the Noncontributory VEAP and Tuition/Stipend cells.

comparison is attributable either to the randomness of the data or to some factor—affecting Navy enlistments only—that was not controlled for in the regression. Combining the two estimates yields a revised spillover estimate of approximately 5 percent.

That an Army program should raise Navy enlistments by 5 percent can readily be explained. An examination of the skill channelling effects of the programs, discussed in the next section, shows that many individuals whose enlistments are attributable to the test programs chose specialties that did not qualify them for the special benefits. Thus it should not be surprising that the Army's advertising and selling of the Ultra-VEAP kicker program would increase enlistments in the other services as well. Young people may be initially attracted to the military by the promise of postservice educational benefits, but that apparently is not the most important consideration in their subsequent choices of service and specialty.

In contrast with the UVK results, which show spillovers from one service into another, the Tuition/Stipend results indicate substantial interservice competition. The enlistment gain that we might have expected to see for the Army has apparently been swamped by cross-service movements resulting from the Army's loss of its competitive advantage in educational benefits. These two sets of results appear to be contradictory. Perhaps we should not be surprised, however, to see the Army losing benefit-motivated recruits in the Tuition/Stipend cell if it cannot seem to hold onto similar recruits in a cell in which it has a considerable advantage. As we will see in the next section, educational benefits seem to move recruits from the Army combat arms into other specialties much more readily than they do in the opposite direction. Eliminating the Army's advantage in educational benefits appears to have led to a flight of benefit-motivated recruits from the Army, which they presumably view as primarily a ground combat force, into the apparently more attractive Navy and Air Force.

Taken together, the two sets of results on interservice effects seem to indicate that educational benefits are not as effective as an enlistment incentive in the Army as they are in the Navy and Air Force. Further support for this hypothesis is found in the relative magnitudes of the Army Ultra-VEAP kicker response and the responses to the Tuition/Stipend program in the Navy and Air Force. These two programs offered approximately the same increment over the respective services' control programs (see Table 2), but the Navy and Air Force programs were much more limited in their coverage and, presumably, were more difficult to sell effectively. However, the reliability of any comparisons of response rates is limited by the statistical uncertainty surrounding the estimates. Substantially larger true dif-

ferences across the services cannot be ruled out, so any conclusions based on observed differences should be viewed as hypotheses.

TRENDS IN PROGRAM EFFECTS

There are a number of reasons why we might expect to observe changes over time in the responses to the various test programs: lags in the programs' implementation, delayed effects of advertising, rapid exhaustion of the pool of potential recruits with an interest in educational benefits, and enlistments during the test period by individuals who previously had rejected military service. Growing responses over time would be evidence that the first two factors were operating, whereas declining responses might indicate the last two. Appendix C gives the results of two attempts to uncover trends in the regression estimates of program effects. Neither attempt revealed statistically significant changes over time, and the inconsistent pattern of the trends—rising in some cases, falling in others—suggests that the observed trends are more an artifact of random fluctuations than they are indicative of underlying change.

The lack of significant trends does not necessarily mean that we have observed long-term, steady-state response rates. As noted in Sec. III, steady-state levels of awareness among high school counselors, parents, and others who influence the decisions of young men may not have been achieved during the test period. Building that awareness of a new program could well take several years, with little or no trend apparent in the first ten months.

NATIONWIDE TRENDS

We saw in Table 7 that the Army and Air Force experienced enlistment gains between the base and test periods, and the Navy a modest decline. This is reflected in the regression intercepts ("constant"), which may be interpreted as the change in the logarithm of nationwide enlistments that would have occurred had there been no test. Converting these to percentage changes yields a 20 percent increase for the Army, a 4 percent fall for the Navy, and a 7 percent gain for the Air Force. Parts of these changes can be attributed to changed recruiting conditions, but the variables included in the model, whose effects are included in the intercept terms, account for only about a 1 percent rise in enlistments. Thus an explanation for the large Army gain must be sought elsewhere. Among the likely candidates are the very substantial policy changes implicit in the adoption of the new

AFQT norms and the imposition of a Congressional mandate calling for fewer low-scoring recruits and more high school graduates. Both of these changes would be expected to affect the Army most strongly, as it was the service with the lowest overall quality in FY80 accessions.[7]

The monthly coefficients suggest that the overall enlistment gains (and losses) relative to the base period were not evenly spread over the ten months of the test. When a more restrictive linear trend is fitted in place of the monthly dummy variables, it indicates a significant trend in the year-to-year gain only for the Navy. In that service, enlistments dropped during the test period by about 3 percent per month, relative to the base period trend. Whether this resulted from an unexplained growth in enlistments during the base period, or a decline during the test period, we could not determine.

LABOR MARKET CONDITIONS AND RECRUITERS

The variables measuring changes in recruiting conditions always enter the regression equations with theoretically correct signs, and their parameter estimates, which measure the elasticities of enlistments with respect to each of the variables, are generally statistically significant.[8] The effects of unemployment and recruiters are the best-identified in the regressions. The elasticity of enlistments with respect to unemployment appears to be about 0.25, although the total responsiveness of enlistments to changing business conditions is larger than is indicated by this elasticity alone because of the apparently independent effects of changes in employment levels and weekly hours. The elasticity for recruiters is about 0.28 in the Army and Navy. The smaller elasticity found for Air Force recruiters has no obvious explanation.

Results for the other three variables—changes in hourly earnings, weekly hours, and total employment—are more mixed. Standard errors are large because these change variables exhibited little variation either over time or across areas.[9] An important finding of this study is that the four related labor market variables do appear to

[7]The Army also reduced its total accession requirements between fiscal years 1980 and 1981. Freeing recruiters to devote more time to pursuing the more desirable classes of recruits may have resulted in greater numbers of high-quality male enlistments.

[8]An elasticity indicates the percentage change in one variable resulting from a 1 percent change in another variable. Although the model estimates these effects by comparing differences across areas, it is reasonable to give the parameter estimates the causal interpretation implicit in the term "elasticity" because the differences compared are in year-to-year changes.

[9]Partial correlation coefficients among the three variables are not large, indicating that collinearity is not the problem.

have independent effects on military enlistments.[10] This study was not able to determine whether this is due to differences in the timing of the movements of these variables over the business cycle, reflects truly different effects on enlistments, or simply indicates that no one alone measures the true earnings prospects of youths.

The indicator variables for the Army's two-year options and for three Navy MORE options show insignificant effects for these FY80 test programs. The variable for the Navy "B" program was omitted from the analysis because its coefficient was both large and of the wrong sign. Effects found for the included indicator variables agree well with those of Haggstrom et al. (1981).

SUMMARY

The regression analysis of the Educational Assistance Test Program yields several important results. First, and in some ways most important, it supported our initial confidence in the quality of the test area assignments. Despite some imbalances that arose during the base and test periods, the regression results and the simpler cross-cell comparisons imply identical conclusions. Some of the estimates of program effects are altered when outside influences are controlled for, but never by more than one or two percentage points.

The test results present a coherent, if perhaps somewhat unexpected, picture of the enlistment effects of the EATP programs. They provide the first definitive evidence that a sufficiently generous program of educational benefits can help the services attract high-quality enlistees. A program need not be available to all enlistees to have this effect: we have seen substantial enlistment responses to narrowly targeted programs. The results also indicate that the contribution requirement of VEAP has not been the culprit in the supposed inability of that program to affect enlistments. If VEAP has a failing, it is more likely to be that the $5,400 government payment that it promises is simply too little to appeal to many young men and women with college aspirations. Finally, the test results indicate that both spillovers and interservice competition were important in determining the overall enlistment responses, in individual services, to the EATP programs.

The next section discusses the skill channelling effects of the test programs. In the concluding section the skill channelling results are

[10]Hours and employment variables have not been included in previous models of enlistments.

combined with the enlistment results of this section to yield some interesting policy implications for the design of any new educational benefits program.

VI. TEST RESULTS: SKILL CHANNELLING

Each of the test programs had the potential to channel high-quality enlistees into the eligible specialties, the designated "critical skills." This is true even where the programs did not appear to have raised the number of high-quality enlistments. The extent of skill channelling is important for two reasons. First, one of the purposes of targeting the test programs on critical skills was to encourage high-quality enlistees to enter those specialties. A targeted program that brings in no new enlistees might still be a cost-effective means of raising the number or quality of enlistees in hard-to-fill specialties. Second, the extent of skill channelling is an important determinant of a targeted program's cost effectiveness as an enlistment incentive. If the program induces a large movement into the eligible specialties, compared with the number of new recruits it brings into the force, it would have a high cost per additional recruit; if the movement is small, the program could be much cheaper than an untargeted program designed to yield the same number of enlistments.

Skill channelling by a program is evidenced by a larger enlistment effect in the eligible specialties than in all specialties, a correspondingly smaller effect (possibly negative) in the ineligible specialties, and thus a rise, compared with the control cell, in the proportion of enlistees entering the program-eligible specialties. In both the Army and the Air Force, the two services for which specialty data were available, these tests show substantial skill channelling. For determining the cost effectiveness compared with an untargeted program, the important question is whether the program has affected enlistments in the ineligible specialties. With an untargeted program, the population eligible for the benefits swells by exactly the number of enlistees brought in by the program; the ineligible skills—nonexistent by definition—cannot be affected. With a targeted program, the eligible population may increase by a larger or smaller number than the increase in total enlistments: if larger, the number of enlistments in the ineligible specialties will decline; if smaller, ineligible-specialty enlistments will rise. On the one hand, not all recruits whose enlistments are attributable to the program will choose an eligible specialty (see the discussion of skill channelling in Sec. III); on the other hand, the availability of the program may affect the specialty decisions of recruits who would have enlisted had there been no program. The

skill channelling results below suggest that the more narrowly targeted is a program, the more will the former effect predominate.

ARMY RESULTS

Table 12 displays counts of high-quality male enlistments in the program-eligible and ineligible specialties during the ten months of the test and base periods, by test cell, and compares the cells in terms of their year-to-year gains. The eligible-specialty counts are the sums of enlistments in the Group I skills over the entire ten-month period and enlistments in Group II skills over the period March to September. Enlistments in Group II skills during December to February are

Table 12

COMPARISONS OF YEAR-TO-YEAR GAINS IN ARMY HIGH-QUALITY MALE ENLISTMENTS: EATP ELIGIBLE SPECIALTIES AND ALL SPECIALTIES

Category/ Test Cell	Enlistment Contracts		Ratio Test to Base	Relative Increase (Percent)
	Base Period	Test Period		
Eligible				
Control	6572	8553	1.3014	—
UVK	2188	3331	1.5224	17.0 (3.8)[a]
NCV	1909	2664	1.3955	7.2 (4.1)
T/S	2012	2720	1.3519	3.9 (4.0)
Total	12681	17268	1.3617	—
Ineligible				
Control	5469	6097	1.1148	—
UVK	2001	2210	1.1044	-0.9 (4.3)
NCV	1840	1892	1.0283	-7.8 (4.5)
T/S	1738	1564	0.8999	-19.3 (4.7)
Total	11048	11763	1.0647	—
All				
Control	12041	14650	1.2167	—
UVK	4189	5541	1.3228	8.7 (2.8)
NCV	3749	4556	1.2153	-0.1 (3.0)
T/S	3750	4284	1.1424	-6.1 (3.0)
Total	23729	29031	1.2234	—

[a]Approximate standard errors are in parentheses.

included in the counts of ineligible specialties. The Group I skills are the primarily combat arms specialties that were eligible throughout the test, and the Group II skills those various specialties that were added in March. The table also reproduces the counts and comparisons for total high-quality male enlistments from Table 7.[1] The standard errors for the program effect estimates in the table are only approximate, but should give a broad indication of their precision.[2]

The estimates of the program effects that appear in the last column suggest that skill channelling did take place. Again, the control cell indicates what would have happened nationwide in the absence of the test programs: a shift toward the program-eligible specialties and away from the ineligible, perhaps because of a deliberate policy change by the Army Recruiting Command, or perhaps because the greater proportion of high-quality recruits among FY81 enlistees forced many of them to choose the less-attractive "critical skills" that happened to be eligible for special educational benefits. In the three test program cells this shift was more pronounced, and of approximately equal magnitude. The Noncontributory VEAP program raised eligible-specialty enlistments about seven percentage points more than it did total enlistments, the Ultra-VEAP kicker program by eight points, and the Tuition/Stipend program by ten points. Although they were ineffective as enlistment incentives, the NCV and T/S programs had substantial skill channelling effects.

The UVK cell presents a picture very much like what we would expect of an untargeted program; all of the enlistment gain appears in the eligible specialties. This does not mean, however, that the processes leading to this result were the same. The results for the other programs strongly suggest that a large part of the 17 percent gain for the eligible specialties under the UVK program consisted of recruits whose specialty choices, but not their decisions to enlist, were affected by the program. The obvious corollary is that many recruits whose enlistments are attributable to the UVK program chose ineligible specialties.

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[1]Although these estimates of the programs' enlistment effects are not as good as those derived from the regression equation presented in the previous section, they are more comparable with the specialty-specific estimates in the top part of the table. Lacking a theoretical model of the specialty selection process, we did not attempt to control for civilian labor market and other influences in a regression model.

[2]Haggstrom et al. (1981, App. G) derive an expression for standard errors in this case, under the assumptions of the Poisson model described in Sec. IV. The errors reported here are based on that expression, but were inflated somewhat because our regression work indicated that the model slightly understated the true variance in the enlistment counts. It should be noted, in addition, that the applicability of the Poisson model to the case of specialty-specific enlistments is not as clear as it is in the case of total enlistments.

A separate examination of the Group I and Group II skills presents a more complex picture of the channelling process. During the December to February period, when only the Group I skills were eligible for the program, the UVK cell outperformed the control cell by 11 percent in total enlistments and by 6 percent in the ineligible specialties, but by only 16 percent in the eligible specialties. It thus appears that the tendency of even benefit-attracted recruits to choose attractive specialties offset the skill channelling effect of the UVK program among recruits who would have enlisted anyway, when the program was limited to the predominantly combat arms specialties of Group I. When the test programs were opened to the Group II skills in March, those specialties experienced an amazing 57 percent enlistment gain in the UVK cell relative to the control cell. For the combat arms skills, however, the relative gain dropped to under 8 percent, about the same as the gain in total enlistments. In other words, during the period March to September the combat arms specialties got only their proportional share of the enlistees brought into the Army by the UVK program; there was no skill channelling into these specialties.

The effect of adding the Group II skills is also illustrated in Table 13, which shows the distribution of high-quality male enlistments among the three specialty groups (I, II, and other), for various periods, in the control and UVK cells.[3] Looking first at the base period we see that the two cells exhibited essentially identical distributions during that period, and in both the early and later parts of the period. Approximately 47 percent of these enlistees entered the Group I skills, 8 to 9 percent the Group II, and 44 percent the other skills. The distribution was slightly different during the early part of the test period, with more enlistees entering the Group I and II specialties. As noted above, in the control cell this change has at least two possible explanations. The larger Group I proportion in the UVK cell, however, indicates that the test program may have yielded a small shift into those specialties.

The addition of the Group II skills to the eligibility list changed the distribution of enlistees. In the control cell, with its program of smaller VEAP kickers, the addition apparently resulted in a shift into the new specialties. Though small in comparison with total enlistments, the shift raised Group II enlistments by 20 percent. Of course, part or all of this shift may have been due to changed recruiting policies, or the relative filling up during previous months of the Group I training

[3]We omit the NCV and T S cells to simplify the table. Data for those two cells yield the same general conclusions.

Table 13

SPECIALTY DISTRIBUTION OF HIGH-QUALITY MALE ENLISTMENTS, BY PERIOD AND CELL
(Percent)

	Base Period		Test Period	
	Control	UVK	Control	UVK
Dec-Feb				
Group I	47.6	46.4	51.7	52.7
Group II	8.1	9.0	10.2	10.4
Other	44.3	44.6	38.1	36.9
(N)[a]	(3281)	(1154)	(4217)	(1642)
Mar-Sep				
Group I	47.6	45.5	48.7	46.6
Group II	8.4	8.9	12.3	16.6
Other	44.0	45.5	39.0	36.8
(N)	(8760)	(3035)	(10433)	(3899)

[a]N = number of enlistees.

slots (the shift came at the expense of Group I).[4] Looking at the UVK cell, however, we see an even larger shift, and most of this larger shift must be attributed to the UVK program.[5] As was the case in the control cell, this shift into the newly opened specialties appears to have come entirely at the expense of Group I jobs. The addition of noncombat jobs—clerks, mechanics, cooks, and others—to the list of specialties eligible for the UVK program caused no new channelling of enlistees from ineligible into eligible skills. The addition did, however, cause many enlistees who would have chosen a combat arms job had the eligibility list been more restricted, to choose instead one of the apparently more attractive Group II jobs. To make matters even worse, there is no evidence (see App. C) that expanding the list raised the total enlistment response to the program.

[4]It is very unlikely that all Group I training slots were filled for the entire year, but if these specialties were well filled compared with others, they would have been offered to potential recruits less often than earlier in the year.

[5]The Army Recruiting Command's job reservation system does not permit regional allocations of training slots. Thus, the UVK cell did not have a set of Group I openings that it could "use up" any faster than the other cells.

AIR FORCE RESULTS

Eligibility for the test programs was much more limited in the Air Force than it was in the Army. During the base period, fewer than 18 percent of high-quality males entered one of the specialties that a year later would be eligible for the Noncontributory VEAP and Tuition/Stipend programs. Table 14 shows that enlistments in these specialties were raised considerably by the two programs, and they were raised by greater percentages than were total enlistments. For the two test cells (NCV and T/S) together, the proportion of total enlistments accounted for by the eligible specialties rose from 17.2 percent in the base period to 19.4 percent during the test. In contrast, the proportion in the control cell rose only from 17.5 to 18.2 percent. Although the differences between eligible- and ineligible-specialty enlistment gains for the test programs may not be statistically

Table 14

COMPARISONS OF YEAR-TO-YEAR GAINS IN AIR FORCE HIGH-QUALITY MALE ENLISTMENTS: EATP ELIGIBLE SPECIALTIES AND ALL SPECIALTIES

Category/ Test Cell	Enlistment Contracts		Ratio Test to Base	Relative Increase (Percent)
	Base Period	Test Period		
Eligible				
Control	2970	3305	1.1128	—
NCV	916	1151	1.2566	12.9 (6.4)[a]
T/S	835	1066	1.2766	14.7 (6.7)
Total	5909[b]	6671	1.1290	—
Ineligible				
Control	14012	14850	1.0598	—
NCV	4179	4492	1.0749	1.4 (3.1)
T/S	4275	4700	1.0994	3.7 (3.1)
Total	27529	29671	1.0778	—
All				
Control	16982	18155	1.0691	—
NCV	5095	5643	1.1076	3.6 (2.8)
T/S	5110	5766	1.1284	5.6 (2.8)
Total	33438	36342	1.0868	—

[a]Approximate standard errors are in parentheses.

[b]Totals include UVK cell.

significant at standard confidence levels, the consistency of the results for the two programs suggests that skill channelling did take place.

The test programs do not appear to have channelled all of the recruits they brought into the Air Force into one of the eligible skills. Although the effects in the ineligible specialties are too small to permit firm conclusions, they indicate that substantial numbers of those recruits whose enlistments are attributable to the test programs entered ineligible specialties. In the case of the Tuition/Stipend program, for example, the 3.7 percent gain in the ineligible specialties, which account for 80 percent of high-quality male enlistees, represents as large a *number* of recruits as does the 14.7 percent gain for the covered specialties. Thus, the eligible-specialty gain accounts for only half of the total enlistment gain, and at least some of the specialty shift must represent the movements of recruits who would have enlisted had there been no special program. We cannot determine whether some individuals who were initially attracted to the Air Force by one of the test programs were so discouraged by the limited set of specialties covered as to not enlist, but those who did enlist appear to have made their specialty choices on much the same basis as any other high-quality recruit.

CONCLUSIONS

Specialty-specif· · data for the Army and Air Force show evidence of skill channelling ffects for the EATP programs. These effects were more pronounced n. the Army, with its high proportion of job openings covered, than in the Air Force. Because the Air Force programs were narrowly targeted, however, the smaller shifts they produced, in terms of numbers, still led to substantial percentage gains in the covered specialties. The extent of channelling was not affected by the Army's addition of noncombat specialties to the eligibility list, but the nature of the channelling was: the enlistment gains in the new skills came at the expense of enlistments in the combat arms. It appears that an educational benefit program must be rather narrowly targeted on the combat arms if it is to have much effect there.

The availability of the test programs affected the specialty choices both of hose recruits whose enlistments were attributable to the programs and of those who would have enlisted had there been no special programs. Particularly noteworthy is the incompleteness of the skill channelling among the former group: only for the Army's Ultra-VEAP kicker program are the data consistent with the hypothesis that all enlistees brought in by the program chose an eligible specialty, and the results for the other Army programs strongly suggest that

the UVK results actually reflect only partial channelling of the benefit-induced enlistees. The Air Force programs appear to have channelled, into the program-eligible specialties, no more than half of the additional enlistees they brought in. Navy specialty data, were they available, would undoubtedly present a similar picture: with only 10 percent of Navy training slots covered, Tuition/Stipend cell enlistments in the eligible specialties would have had to have doubled—a very unlikely event—to absorb all the recruits brought into the Navy by that program.

Viewed strictly as an enlistment incentive, a targeted program might seem to have an advantage over an untargeted program because targeting limits the size of the group that might receive economic rents—payments to individuals who would have enlisted had there been no program. Whether a targeted program is in fact superior, however, depends on the answers to two questions. First, does targeting discourage enlistments? The test offers no direct evidence on this point, but the substantial enlistment effects of the very narrowly targeted Tuition/Stipend program in the Navy and Air Force suggest that, at the very least, targeting does not reduce enlistments in proportion to the size of the targeted group. Second, does targeting discourage qualifying for benefits? Here, the answer is a clear affirmative. So many recruits were discouraged that for none of the programs that had any appreciable enlistment effect was there any inflow into the covered specialties, net of the gain in total enlistments. An untargeted program necessarily expands the pool of potential recipients by exactly the amount of its enlistment effect: these targeted programs not only created no more expansion, when narrowly targeted they created even less. Undoubtedly, this result was due in part to the availability of basic VEAP in the specialties not covered by the test programs, so it should not be expected to hold if educational benefits were available only in selected specialties. For a two-tier program of the type implicitly tested here, however, narrow targeting appears to offer a means for limiting payment of the larger benefit to those recruits most strongly committed to obtaining it. While not conclusive, the skill channelling results definitely suggest that a targeted program of educational benefits would cost less, per additional recruit attracted, than one that is untargeted.

VII. CONCLUSIONS

No one knew, when termination of GI Bill educational benefits was being considered in 1976, what the effect would be on military enlistments. Though it was widely lauded as good for veterans, for the services, and for the country, the GI Bill was seen as an expensive holdover from the era of conscription. Under the draft, educational benefits were viewed as a way to compensate young men for their two years of hard, low-paid service to country; to help restore the educational opportunities they had lost as a result of their service; to assist in their readjustment to civilian life; to improve the educational attainment of the nation as a whole; and last, and probably least, to help the services recruit volunteers. With the draft ended, and military pay raised to a level comparable with pay in the civilian economy, many of these goals seemed less important. Why should a volunteer, who for his own reasons decides that the military is a good place to spend three or more years of his life, require all these forms of assistance? Why indeed, the Congress answered, and did away with the GI Bill.

The principal defect that the Congress saw in the GI Bill was its cost. Members were not convinced that its goals were made obsolete by the ending of the draft, so veterans' educational assistance was not eliminated, simply reduced, and a requirement was added that to receive government assistance servicemembers set aside some of their military pay to help finance their education. Benefit levels were greatly reduced; instead of the $270 per month or more, for up to 45 months, that the GI Bill was offering in 1976, the new Veterans Educational Assistance Program (VEAP) offered a maximum of $150 per month for 36 months. The saving, or "contribution," requirement of the new plan was expected to further reduce costs by limiting payments to those veterans who were most committed to pursuing higher education. The adoption of VEAP set the stage for continuing debate on educational benefits, ultimately leading to the 1981 Educational Assistance Test Program.

POLICY DEBATE AND THE TEST

VEAP was by some accounts a failure. Participation in the plan during its first few years was low—less than 25 percent of 1978 enlistees enrolled—and the services found themselves unable to attract as

many high-quality recruits as they desired. Whether these recruiting difficulties were a product of the change in educational assistance plans we may never know, but by 1978 Congress was reconsidering its earlier cost-cutting measures. It directed the Department of Defense to conduct an experiment, during 1979, to determine the effectiveness of enhancements to the VEAP offering, and a two-year enlistment option, for attracting high-quality enlistees to the Army and Navy.

The Multiple Option Recruiting Experiment (MORE) yielded inconclusive results. The VEAP enhancements of $2,000 to $4,000, called "kickers," appeared to raise high-quality male enlistments by about 7 percent. Limitations of the design left considerable uncertainty about the precise amount of this increase, but MORE did provide the first experimental evidence that educational assistance could be an effective inducement for young men to enlist.

The 1981 Educational Assistance Test Program (EATP) was born in part out of the inconclusiveness of the MORE results. Convinced that the MORE kickers were simply too small to yield a measurable response, the Army requested a test of larger kickers. At the same time, the House was proceeding with plans for a test of a new program—one based on the model of the World War II GI Bill, with its separate tuition assistance and subsistence allowance components. The Senate Armed Services Committee, however, developed a less generous plan which merely eliminated the VEAP contribution requirement. The competing plans were reconciled in conference by a simple expedient: the Department of Defense was told to test both.

Three plans were thus to be tested in the EATP: larger kickers of $8,000 to $12,000 for the Army only (Ultra-VEAP kickers), a Noncontributory VEAP plan, and a program that some have called the Tuition/Stipend plan. All three were more generous than the program in place during 1980: basic VEAP for all the services, plus $2,000 to $6,000 kickers for certain high-quality Army enlistees. All three were restricted, in the test design, to high school graduates of above average aptitude who enlisted for certain specified "critical skills." A fourth plan, serving as the control program, retained the 1980 offering of each of the services. Only one of the four programs, the Tuition/Stipend Plan, limited the Army to the same level of benefits as the other services.

The test design offered each of the programs in a set of geographically dispersed areas of the country, and the test cells were made large enough to give the test results a desired degree of precision. The test began on December 1, 1980, and ended on September 30, 1981.

Three important limitations of the test should be borne in mind. First, the test comprised complex experimental treatments; indeed, some of the programs posed more questions than could be answered.

The Tuition/Stipend plan, in particular, differed from the control program in many respects. Although the test could show the impact of all of these features together, it could not unravel their separate effects. Second, all of the programs restricted the new educational benefits to certain types of enlistees: high-quality recruits who were willing to enter one of the designated critical skills. Even though some of the test results carry implications that bear on the effectiveness of a general entitlement program, this test was not designed to demonstrate the effects of a universal program open to all enlistees. Third, awareness of the programs among potential recruits and those who influence their decisions may have been lower than would be the case for a permanent program, and advertising undertaken to correct this problem may have had a direct effect on enlistments. Thus, the "program effects" found may understate the long-term steady-state effects of the programs and, perhaps offsetting this, may include direct effects of the greater advertising expenditures in the test cells.

TEST RESULTS

Despite its limitations, the Educational Assistance Test Program yielded several important results. Not the least of these is the clear indication of the efficacy of controlled experiments for determining the effects of proposed new programs. Had this quality been better recognized in 1976, perhaps the effects of educational benefits could have been demonstrated much earlier.[1] In addition, the test showed why an adequate experimental control group is important. Lacking one in the EATP, we might have been led incorrectly to attribute the Army enlistment gains of 14 to 32 percent, between 1980 and 1981, entirely to the three test programs.

The test also yielded some more immediately useful results. First, it provided the only strong evidence that a sufficiently generous program of veterans' educational benefits can bring high-quality recruits into the military. The Army's Ultra-VEAP kicker program raised Army enlistments of high-quality males by about 9 percent, and not at the expense of enlistments in the other services. This is a considerably larger enlistment gain than theoretical examinations of the program have indicated would occur.[2]

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[1]VEAP was enacted as a five-year "test," but with no control provided it was impossible to determine its effects.

[2]See, for example, Congressional Budget Office (1982). Section III describes the CBO methodology for estimating enlistment effects and points out its limitations.

Second, the test results suggest that the contribution requirement of VEAP has not resulted in significant enlistment losses. The Noncontributory VEAP program yielded only small enlistment gains—virtually none in the Army, where the program was most generally available, and only in the Air Force was the response statistically different from zero. This is not to say that a switch to a noncontributory program would not help recruiting, but it does suggest that the costs of such a switch should be examined very carefully before it is made.

Third, the results indicate that giving the Army a more generous program than the other services is not simply a way of helping the Army while keeping costs down; putting the services on an equal footing, even with a program that is more generous than the Army's current offering, might actually hurt Army recruiting. This conclusion is based on the enlistment results for the Tuition/Stipend program, which was more generous than the Army's control program but gave all the services the same maximum benefit. It reduced Army high-quality male enlistments while increasing enlistments in the Navy and Air Force. This result is, of course, for one particular program, limited to certain enlistees, and implemented on a test basis. A permanent general entitlement might have a greater market expansion effect, large enough to offset the cross-service movements and leave the Army with a net enlistment gain. Even were this to happen, however, it appears that the Army would be least helped by any new educational benefits program that did not maintain the current differential among the services.

Fourth, the test has shown that a program of educational benefits need not be open to all enlistees to have an impact on total enlistments. In the Air Force, the specialties open to the test programs accounted for less than 20 percent of 1980 high-quality male enlistments, and in the Navy for only about 10 percent, yet both services showed substantial enlistment gains under the Tuition/Stipend program. The gains were so large, in fact, that it is quite clear that many of those recruits whose enlistments were attributable to this program chose specialties that did not qualify them for the special benefits.

Finally—a point related to the previous one--it is now apparent that educational benefits can be used to channel high-quality enlistees into specific skills (hardly surprising), but the extent of that channelling depends on both the proportion of specialties open to the program and their nature. In the Army, the service with the greatest number of specialties eligible for the test programs, the number of enlistees added to covered specialties under the Ultra-VEAP kicker program was as large as the total enlistment gain for the program. In the Air Force this was not the case. In addition, the expansion of the Army list of eligible specialties in the middle of the test yielded an

unexpected result. The new jobs, none of them combat related, drew many enlistees away from the combat arms and the addition of the jobs had no apparent effect on total high-quality enlistments. An Army educational benefit program apparently must be very tightly focused if it is to help the combat arms.

The last result—what happened when the Army list was expanded—suggests part of the explanation for the Army's poor showing under the Tuition/Stipend program. With its image as a service offering primarily ground combat jobs, the Army may be viewed less favorably among those who are attracted to the military by educational benefits than among those who enter for other reasons. If this is the case—and the test results here are only suggestive—it strengthens the expectation that a new GI Bill would not help Army recruiting as much as it would the recruiting of the Navy and Air Force.

COST CONSIDERATIONS

Interpretations of the test results are strengthened by a simple examination of costs. A complete examination of costs is not currently possible because it would require information on participation and benefit usage under VEAP and the VEAP kickers, information that will not be available until those programs have been in place for several more years. For now, we must be content with indications of directions, rather than magnitudes. It appears that two features of some or all of the tested programs could be used to reduce the cost of an educational benefit program: targeting on certain specialties and a contribution requirement. A third feature, targeting on high-quality enlistees, also has cost-saving implications, but these were not addressed by the test.

Targeted programs are generally seen as a way to minimize costs by focusing benefit dollars where they are most required, but this is too narrow a view. A targeted program may be more cost effective than an untargeted even when viewed solely as an enlistment incentive. We saw in the Navy and Air Force results that a targeted program can raise total enlistments by an amount that is out of proportion to the limited coverage of the eligible specialties. We saw further that the enlistment gains of a narrowly targeted program are not accompanied by a large shift into the covered specialties. Indeed, narrow targeting appears to spread the total enlistment gain across both covered and uncovered specialties, discouraging even those enlistees who were initially attracted to the service by the benefits from choosing a qualifying skill. With an untargeted program, of course, all en-

listees are in eligible specialties. A targeted program would appear to be more cost effective because targeting does not seem to discourage many potential recruits from enlisting, but it does discourage participation.

The cost advantage of a contributory program is clear, and the test results show that the cost savings are not offset by a corresponding reduction in enlistments.[3] This result is confirmed by the survey data reported by Orvis and Hawes (forthcoming). A contribution requirement offers additional cost savings, however, savings that are perhaps less widely recognized, when the benefit program offers special enhancements to certain enlistees. Earning of the kickers tested in EATP and MORE has always been tied to participation in basic VEAP. By discouraging from the kicker program those enlistees who are only marginally committed to pursuing postservice education, the contribution requirement limits the payment of rents. Kickers will go primarily to those whose interest in education is so strong that they would not have enlisted had there been no kicker program.

A contribution requirement may be justly criticized if it discourages from participation those enlistees who can benefit most from higher education, or whom society would most like to see get that education. Although it was not a stated goal of the GI Bill to help the disadvantaged, or to raise the general level of education, these purposes have certainly been raised in discussions of military educational benefits. It is beyond the scope of this study to evaluate these arguments, but we can note that blacks and Hispanics were overrepresented among participants in VEAP during its first two years.

One positive effect of a contribution requirement is that it discourages use of education services as consumption goods. Under the GI Bill, the cost to veterans of many courses of study was zero or even negative. As a result, we saw examples of courses that were undertaken not to raise the individual's earning potential, but simply because he wanted to build a color television, or learn to fly, or merely to collect the extra money the GI Bill provided. There is nothing wrong with such activities *per se*, but the arguments that are generally raised in support of government subsidies to education—capital markets are imperfect, society as a whole captures some of the gains from an individual's education—do not apply to education undertaken for its immediate consumption value. An in-service contribution require-

[3]Indeed, the results make it appear that potential recruits are deterred neither by the requirement that they temporarily forgo income nor by the provision that one-third of the $8,100 maximum benefit promised by VEAP must come out of their own pockets.

ment does not eliminate these activities, but it is likely to reduce them.

The Congressional Budget Office (1982) has recently estimated the long-term costs of four alternative programs, two of which closely resemble the Ultra-VEAP kicker and Noncontributory VEAP plans tested in EATP. Continuing the Army's $8,000 to $12,000 kickers, with only basic VEAP for the other three services, would result in a steady-state annual program cost of $139 million (1983 dollars), according to CBO. Removing the contribution requirement of that plan would increase the annual cost fivefold, to $720 million. Although CBO is more optimistic about the enlistment effect of removing the contribution requirement than our test results indicate they should be, their calculations show a more than threefold increase in the cost per additional recruit attracted by the educational benefit programs.

POLICY IMPLICATIONS

The test results, and these simple cost considerations, have several implications for future policy.

First, serious consideration should be given to retaining a contribution requirement in any new military educational benefits program. Low participation rates in VEAP have been cited as evidence that the program has been ineffective, but they could instead be taken as indicating that the contributory feature of VEAP has accomplished its purpose of reducing program costs while ensuring that the limited funds available for educational assistance have gone to those enlistees with the greatest commitments to further education. Even if a modest noncontributory program is adopted, special enhancements for selected enlistees, which could be made the principal drawing cards of the program, could be designed to include a contributory feature.

Second, any program should be skill-targeted. Targeted programs bring in more enlistees than might be expected given the proportion of specialties covered, and they may bring in as many as would be attracted by a general program offering the same maximum benefit. A complete examination of costs almost certainly would reveal a targeted program to be more cost effective as an enlistment incentive than an untargeted program. Young people apparently react to the maximum benefit available, regardless of whether the program is targeted or not, contributory or noncontributory. When they find that they must carry a rifle to earn that maximum benefit, rather than run a computer, most apparently do not respond by not enlisting, and the choice of whether to take the higher benefit by selecting one of the targeted specialties ultimately lies with them. Keeping the list of

covered specialties short—and particularly restricting it to the less attractive hard-to-fill skills—with a much more modest benefit for other jobs, will ensure that most of the dollars spent will go to those enlistees who most want to continue their education, and whose decisions to enlist were most influenced by the availability of postservice educational benefits.

Finally, in the design of a new program some recognition must be made of the special problems faced by the Army. Youths who are motivated to enlist by the promise of educational benefits seem to be less willing than others to serve in the ground combat arms. Given a choice, they will choose a noncombat job over a combat job within the Army, and more generally will choose the Navy or Air Force over the Army. To help the Army with a targeted educational benefits program, it is not enough to give the Army a longer list of covered specialties. This was made clear in the Tuition/Stipend cell, where the Army offered the program to three times as many recruits as the Air Force, and six times as many as the Navy, yet apparently lost recruits to both services. A new program that did not allow the Army to offer greater maximum benefits than the other services—even a program more generous than the current—would reduce the Army's share, and perhaps even its number, of high-quality enlistments.

The Army's special problem does not necessarily mean that any extra help it is given should come from more generous *educational* benefits than the other services. If educational benefits are indeed a less effective means of attracting recruits to the Army than to the Navy and Air Force, other incentives should be considered. A bonus for three-year Army enlistees, for example, might be combined with a skill-targeted educational benefit program. This assumes, of course, that the Army currently needs special help, which is less clear now than it was in 1980. High-quality male enlistments in the Army were up substantially in fiscal year 1981 over their 1980 levels, and results for fiscal year 1982 indicate that the improvement in the quality of Army enlistees is continuing.

These conclusions do not imply that a new educational benefit program, or even the retention of the existing VEAP (plus Army kickers), is necessarily desirable. There are as yet no good estimates of the costs of such programs. The recent Congressional Budget Office estimates appear to be the best available, but their reliability is questionable because they depend upon assumptions about enlistment effects that appear to be understated, given the EATP results. CBO also was forced to estimate costs based on GI Bill usage rates, which may not give accurate indications of usage by VEAP and VEAP-kicker participants. Within the next few years, data will become available on benefit payments to kicker-eligible veterans who enlisted during the 1979

experiment. Until those data are analyzed, we will remain uncertain about the balance of cost and benefits for a program of the type we suggest, although it should be cost superior to the noncontributory, untargeted programs generally being considered. In the meantime, we should examine closely the effectiveness and costs of alternative means for bringing highly qualified individuals into the military services.

A new GI Bill has been advocated as a way to save the All Volunteer Force, to reward service to country, and to turn military service from a job into an opportunity for advancement. The Educational Assistance Test Program could not reveal whether a GI Bill would accomplish these purposes. What it has shown is that educational assistance tied to service in the military does have some appeal to today's youth, and can assist the services in their efforts to attract highly qualified recruits. It has shown, further, a way to accomplish many of the recruiting objectives of a new GI Bill without the high costs usually estimated for such a program: restrict the maximum payment to enlistees in a limited number of military specialties, and require the enlistee to demonstrate his commitment to study by setting aside some of his military pay to help with college expenses. Two important questions remain: (1) Would some alternative enlistment incentive be a more cost-effective means of helping the services attract quality recruits and (2) does the experience of the 1980s mean that such help, which seemed so obviously needed in the late 1970s, is no longer required? These issues may well be the focus of debate on any new program of military educational assistance.

Appendix A

MILITARY SPECIALTIES ELIGIBLE FOR TEST PROGRAMS

Table A.1

AIR FORCE SPECIALTIES ELIGIBLE FOR TEST PROGRAMS

AFSC	Title
20731[a]	Morse System Operator
20732[a]	Printer Systems Operator
20830[a]	Voice Processing Specialist
46130	Munitions Systems Specialist
46230	Aircraft Armament Systems Specialist (A, B, C, D, E, F, G, H, J, Z)
46430[a]	Explosive Ordnance Disposal Specialist
81130	Security Specialist

[a]Also eligible for enlistment bonus, as of December 1, 1980.

Table A.2

NAVY SPECIALTIES ELIGIBLE FOR TEST PROGRAMS

Rating	Title
STS[a]	SONAR Technician (Submarine)
STG[a]	SONAR Technician (Surface)
CTI[a]	Cryptologic Technician Interpretive
TMT[a]	Torpedoman's Mate (Technical)
ET[a]	Electronics Technician — Nuclear Field
EM[a]	Electrician's Mate — Nuclear Field
IC[a]	Interior Communications Electrician — Nuclear Field
TM(SS)[a]	Torpedoman's Mate (Submarine)
RM	Radioman
SM[a]	Signalman
CTT[a]	Cryptologic Technician Technical
CTR[a]	Cryptologic Technician Collection

[a]Also eligible for enlistment bonus, as of December 1, 1980.

Table A.3

Army Specialties Eligible for Test Programs--Group I

MOS	Title
05B[a]	Radio Operator
05C[a]	Radio Teletype Operator
05D[a]	EW/SIGINT Identifier/Locator
05G[a]	Signal Security Specialist
05H[a]	EW/SIGINT Morse Interceptor
05K[a]	EW/SIGINT Non-Morse Interceptor
11B[a]	Infantryman
11C[a]	Indirect Fire Infantryman
11H[a]	Heavy Antiarmor Weapons Crewman
12B[a]	Combat Engineer
12C	Bridge Crewman
12E[a]	Atomic Demolotions Munitions Specialist
13B[a]	Cannon Crewman
13C[a]	TACFIRE Operations Specialist
13E[a]	Cannon Fire Direction Specialist
13F[a]	Fire Support Specialist
13R[a]	Field Artillery Firefinder Radar Operator
15D[a]	LANCE Missile Crewman
15E[a]	PERSHING Missile Crewman
15J[a]	LANCE/HONEST JOHN Operations/Fire Direction Specialist
16B	HERCULES Missile Crew Member
16C[a]	HERCULES Fire Control Crew Member
16D[a]	HAWK Missile Crew Member
16H	ADA Operations and Intelligence Assistance
16J	Defense Acquisition Radar Operator
16P	ADA Short Range Missile Crewman
16R	ADA Short Range Gunnery Crewman
17B	Field Artillery Radar Crew Member
17C	Field Artillery Target Acquisition Specialist
19D[a]	Cavalry Scout
19E[a]	M48-M60A1/A3 Armor Crewman
19F[a]	Tank Driver
19J[a]	M60A2 Armor Crewman
19K	XM-1 Armor Crewman
19L	XM-1 Tank Driver
36K	Tactical Wire Operations Specialist
45K	Tank Turret Repairer
45N	Tank Turret Mechanic
54E[a]	Chemical Operations Specialist
55B	Ammunition Specialist
55G	Nuclear Weapons Maintenance Specialist
72E	Telecommunications Center Operator
82C	Field Artillery Survey
95B	Military Police
96C[a]	Interrogator
98G[a]	EW/SIGINT Voice Interceptor

[a]Also eligible for enlistment bonus, as of January 28, 1981.

Table A.4

Army Specialties Eligible for Test Programs—Group II

MOS	Title
03C	Physical Activities Specialist
21G[a]	PERSHING Electronics Material Specialist
35G	Biomedical Equipment Repairer
42D	Dental Laboratory Specialist
63B	Power Generation and Wheel Vehicle Mechanic
64C	Motor Transport Operator
71D	Legal Clerk
71L	Administrative Specialist
71M	Chapel Activities Specialist
71R	Broadcast Journalist
72G	Data Communications Switching Center Specialist
75B	Personnel Administration Specialist
76C	Equipment Records and Parts Specialist
76Y	Unit Supply Specialist
82B[a]	Construction Surveyor
82D[a]	Topographic Surveyor
91E	Dental Specialist
93H	Air Traffic Control Tower Operator
94B	Food Service Specialist
98J	EW/SIGINT Noncommunications Interceptor

[a]Also eligible for enlistment bonus, as of January 28, 1981.

Appendix B

AFEES TEST CELL ASSIGNMENTS

Control

Albany, NY
Anchorage, AK
Baltimore, MD
Boise, ID
Boston, MA
Butte, MT
Chicago, IL
Cleveland, OH
Columbus, OH
Denver, CO
Des Moines, IA
Guam
Harrisburg, PA
Honolulu, HA
Indianapolis, IN
Jacksonville, FL
Knoxville, TN
Little Rock, AR
Los Angeles, CA
Manchester, NH
Montgomery, AL
Nashville, TN
New Haven, CT
New Orleans, LA
Phoenix, AZ
Pittsburgh, PA
Portland, ME
Richmond, VA
Salt Lake City, UT
San Diego, CA
San Juan, PR
Shreveport, LA
Springfield, MA
St. Louis, MO
Wilkes-Barre, PA

Ultra-VEAP Kicker

Atlanta, GA
Cincinatti, OH
Coral Gables, FL
Fort Hamilton, NY
Fresno, CA
Kansas City, KS
Minneapolis, MN
Newark, NJ
Oakland, CA

Noncontributory VEAP

Albuquerque, NM
Amarillo, TX
Charlotte, NC
Detroit, MI
El Paso, TX
Fort Jackson, SC
Milwaukee, WI
Oklahoma City, OK
Portland, OR
Raleigh, NC
San Antonio, TX

Tuition/Stipend

Beckley, WV
Buffalo, NY
Dallas, TX
Fargo, ND
Houston, TX
Jackson, MS
Louisville, KY
Memphis, TN
Omaha, NE
Philadelphia, PA
Seattle, WA
Sioux Falls, SD
Spokane, WA
Syracuse, NY

Appendix C

CHANGES OVER TIME IN TEST PROGRAM EFFECTS

Implicit in our use in the regression model of a single variable to capture the enlistment effect of each of the test programs is the assumption that the programs' effects were stationary over time. To test whether this assumption was correct, we reestimated the model for each of the services with additional variables to measure any changes. Two approaches were followed: indicator variables and linear trend.

In the indicator variables approach, the ten-month test period was divided into two subperiods: December-February and March-September. Indicator variables were introduced into the model for the latter period in each of the three test program cells. That is, the added variable for the UVK cell, for example, took on the value one if the observation was on an AFEES in the UVK cell *and* from a month in the period March to September; for all other AFEES, and for all AFEES from December to February, the variable took on the value zero. This gave the coefficient estimates for the original program indicators the interpretation of the effects of the programs from December to February, and for the new variables the interpretation of the *change* in the effect between the earlier period and the later period. Standard significance tests for the latter coefficients then indicate whether the measured changes are statistically different from zero.

The unequal division of the test period was borne of an expectation that it might take several months after the introduction of the test programs before their full effects would be felt. The details of the test were not set until November 1980. As a result, Joint Recruiting and Advertising Program (JRAP) advertising of the Noncontributory VEAP and Tuition/Stipend programs did not begin until January, and Army instruction manuals on the test programs did not reach recruiters until late January.[1] The particular division of the test period—between February and March—had another rationale; it was at the end of February that the Army's Group II specialties were added to the eligibility list. We also estimated the model with the second period further subdivided, in case the response lags were

[1]Brief descriptions of the programs were sent to Army recruiters when the test began.

longer than we expected or the programs' effects eventually diminished.

In the linear trend approach, a variable was introduced for each cell that took on the value −4.5 in December, −3.5 in January, and so on to +4.5 in September. Centering these variables on zero gives the coefficients for the program indicator variables the interpretation of (approximately) the average program effects over the entire ten months.[2] Coefficients for the trend terms indicate how much the program effects changed from one month to the next, under the assumption that the changes were constant.

Table C.1 gives the coefficient estimates for the program and trend terms only.[3] Results in the top panel are for the indicator variable approach, and results in the bottom are for the linear trend approach. Looking at the Army UVK results, for example, we see in the upper panel that during the December to February period, the program raised the logarithm of enlistments by 0.11 (11.6 percent), but that the response fell off by 0.034, to about 7.9 percent, during March to September. This fall is decomposed into: (1) a small rise (0.022) in March-May and (2) a drop of 0.069 in June-September. The large standard errors for the change coefficients indicate, however, that the results are consistent with an enlistment effect that did not change over time, as well as a substantial fall or rise in the true program effect. In the bottom panel we see an average effect of 0.088 (9.2 percent)—almost exactly the same as is reported in Table 10—with a fall of about 1 percent per month. Again, the coefficient for the monthly change is not significantly different from zero at the 5 percent level.

Neither approach yields indications of any statistically significant changes over time in the programs' effects. In large part, however, this is due to the inherent randomness of the data. Changes between periods would have had to have been at least 12 percentage points to be statistically different from zero, and month-to-month changes would have had to have averaged almost 2 percentage points. The results are worth examining, therefore, for any consistent story they might tell.

With few exceptions, one is hard pressed to find any readily explainable phenomena in the results. Some trends are up, some are down; for no program are the trends in the same direction in all three services, and only the Navy had the same general trend in all three

[2]The program coefficients are only approximately the average effects over all ten months because the model was estimated by weighted least squares. To the extent that enlistments (which determine the weights) exhibit seasonality, the average program effect estimates of this model will differ slightly from those in Table 10.

[3]Estimates for the other variables in the model were essentially the same as those presented in Table 10.

Table C.1

Changes over Time in Test Program Effects (High-Quality Males)

	Army		Navy		Air Force	
Period indicator variables						
UVK						
Dec-Feb[b]	0.110[a]	(0.051)	0.009	(0.056)	-0.007	(0.045)
Mar-Sep[b]	-0.034	(0.061)	0.112	(0.065)	0.048	(0.053)
Mar-May[b]	0.022	(0.072)	0.128	(0.076)	0.034	(0.064)
Jun-Sep[b]	-0.069	(0.066)	0.102	(0.069)	0.057	(0.058)
NCV						
Dec-Feb[b]	0.059	(0.056)	-0.029	(0.054)	0.098[a]	(0.049)
Mar-Sep[b]	-0.065	(0.066)	0.090	(0.064)	-0.061	(0.058)
Mar-May[b]	-0.068	(0.078)	0.091	(0.077)	-0.060	(0.069)
Jun-Sep[b]	-0.062	(0.072)	0.088	(0.069)	-0.061	(0.064)
T/S						
Dec-Feb	-0.096	(0.054)	0.067	(0.054)	0.137[a]	(0.050)
Mar-Sep	0.054	(0.064)	0.046	(0.064)	-0.086	(0.058)
Mar-May[b]	0.098	(0.078)	0.038	(0.076)	-0.082	(0.069)
Jun-Sep[b]	0.025	(0.070)	0.088	(0.070)	-0.088	(0.063)
Linear trend						
UVK						
Average	0.088[a]	(0.028)	0.088[a]	(0.028)	0.025	(0.024)
Trend	-0.011	(0.010)	0.013	(0.010)	0.009	(0.008)
NCV						
Average	0.013	(0.029)	0.034	(0.029)	0.056[a]	(0.026)
Trend	-0.003	(0.010)	0.010	(0.010)	-0.007	(0.009)
T/S						
Average	-0.059[a]	(0.030)	0.098[a]	(0.030)	0.078[a]	(0.027)
Trend	0.005	(0.010)	0.008	(0.010)	-0.010	(0.009)

[a]Indicates t-ratio exceeding 1.96 in absolute value. Standard errors in parentheses.

[b]Coefficient indicates change compared with December-February.

programs. The Navy trends may indicate that this one service did have trouble implementing the test programs, but noting that the enlistment growth took place in the Ultra-VEAP kicker cell, as well as in those cells where the Navy had test programs, we might just as easily interpret the results as indicating a minor downtrend in control cell enlistments. The Army results give no support for the hypothesis that expanding the Army's list of eligible specialties improved the appeal of the Army programs.

The lack of any consistent pattern in the trends strongly suggests that the observed changes are more a product of random fluctuations in enlistments than an indication of true changes over time in the enlistment effects of the test programs.

Appendix D

FY82: ADDITIONAL INFORMATION ON THE ULTRA-VEAP KICKER

At the end of fiscal year 1981, the Noncontributory VEAP and Tuition/Stipend programs expired in all the services, but the Army UVK program, renamed the Army College Fund, was extended nationwide. Navy, Air Force, and Marine Corps educational benefit programs reverted to the basic VEAP plan in all areas. Thus, FY82 provides, with certain limitations, a second "baseline" period, in the sense that all areas of the country had the same educational benefit offerings. Enlistment data from this second baseline may be compared with FY81 data to yield additional estimates of recruiting performance during the period of the Educational Assistance Test Program.

Three factors prevent a complete assessment of the test programs' effects compared with a FY82 baseline. First, the Army began a new experiment with enlistment bonuses in July 1982, effectively ruling out the use of data from July through September for analyzing the EATP. Second, in the Noncontributory VEAP and Tuition/Stipend cells, two changes took place between FY81 and FY82: (1) the test programs were eliminated, and (2) the UVK program was added for the Army. Comparisons involving these two cells would confound the effects of the two changes. Third, awareness of the programs on the part of potential recruits, built up during the test period, might be expected to affect enlistments during FY82. Again, comparisions involving the two eliminated programs—Noncontributory VEAP and Tuition/Stipend—would be most seriously affected. Neither of the latter two problems, however, should affect either the control or the Ultra-VEAP kicker cells.

By comparing the FY82 results through June with FY81 data it is possible to form a second set of estimates of the effects of the UVK program on Army and other-service enlistments. Of particular interest is the effect on Navy enlistments: the FY80-FY81 comparison yielded an apparent "spillover" that was disconcertingly large. The two sets of estimates will not be independent, of course, because they share the test period data, but the second set will provide additional information.

The change in the EATP control cell—the UVK program substituted for the previous control program—alters somewhat the appropriate comparison for determining the UVK effects. Now it is the EATP con-

trol cell that has experienced a change, whereas the UVK cell of EATP serves as a control (unchanged between FY81 and FY82). Thus, if the EATP control cell experienced a larger enlistment gain between the FY81 test period and the same months of FY82 than did the UVK cell, we may interpret the extent of that outperformance as a measure of the effect of the UVK program in the areas where it was added. That is, the estimate of the UVK program effect is the ratio of FY82 to FY81 enlistments in the EATP control cell, divided by the same ratio for the UVK cell. Data were not available to permit regression controls for outside influences, but based on the small changes such controls made for the FY81-FY80 comparison, we can use the simple comparisons of cell totals without fear that they might be seriously biased.

Table D.1 gives the FY81 and FY82 data for the months December through June: counts of high-quality male enlistments in the control and UVK cells, the year-to-year ratio of those counts, and the estimated UVK program effect in FY82.[1] As was the case between FY80 and FY81, the Army experienced a large gain in high-quality male enlistments. These data, combined with those in Table 7, suggest that over the two-year period FY80 to FY82, the Army would have registered a 79 percent increase even in the absence of the new educational benefit programs. The Navy and Air Force reversed their relative positions of the previous comparisions, so that each recorded a small gain between FY80 and FY82.

The column headed "relative increase" gives the estimated program effects. Of the three services, only the Army showed a substantial relative gain in the control cell, where the UVK program was added in FY82. The 8.6 percent estimate is remarkably close to the 9.1 percent from the regression results (Table 11), and the 8.7 percent reported in Table 7.[2] The Air Force result is also consistent with the earlier findings: a small, possibly nonexistent, spillover.

The Navy result is markedly different. Instead of indicating a large spillover effect of the Army program into Navy enlistments, the FY82 data show a very slight negative effect. The magnitude of the difference between the two estimates, and the consistency of the results for the Army and Air Force, suggest that the earlier Navy result was truly an anomaly, due either to the basic randomness of the data or to

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[1]The comparisons exclude October and November (recall that the UVK test began in October 1980) to be more nearly comparable with the results presented in Secs. IV and V. Including those two months does not appreciably alter the comparisons.

[2]For comparison, using only the December-June data for FY80 and FY81 yields an estimated UVK effect (without regression controls) of 10.5 percent. As shown in App. C, the effect of the UVK program on Army enlistments diminished somewhat (though not significantly) during the summer months of FY81.

Table D.1

COMPARISONS OF YEAR-TO-YEAR GAINS IN HIGH-QUALITY MALE ENLISTMENTS: FY81 AND FY82

Service/ Test Cell[a]	Enlistment Contracts FY81 Dec-Jun	Enlistment Contracts FY82 Dec-Jun	Ratio FY82 to FY81	Relative Increase (Percent)
Army				
Control	9710	15442	1.5903	8.60(2.97)[b]
UVK	3734	5468	1.4644	—
Navy				
Control	9148	10798	1.1804	-1.54(3.55)
UVK	3929	4710	1.1988	—
Air Force				
Control	12143	11381	0.9372	2.84(3.17)
UVK	4570	4165	0.9114	—

[a]Cells are indicated by EATP designations. The Army control cell offered $2000, $4000, and $6000 kickers in FY81, Ultra-VEAP kickers ($8000, $12000, and $12000) in FY82. The Army UVK cell offered Ultra-VEAP kickers in both periods. The Navy and Air Force offered only basic VEAP in both cells during both periods.

[b]Approximate standard errors are in parentheses.

some factor—affecting Navy enlistments only—that was not controlled for in the regression. This is confirmed in another examination of the data, comparing the control and UVK cells in terms of their enlistment growth between FY80 and FY82. We should expect these growth rates to be essentially the same, since in each period the educational benefit offerings were identical in the two cells. In the Army and Air Force the cells' growth rates differed by about three percentage points, but in the Navy the difference was almost 12 percentage points.[3] It is impossible to know, of course, whether Navy enlistments were unusually low in the UVK cell during FY80, or unusually high during FY82 (the control cell growth is subject to less randomness because of the larger number of enlistments involved), but we are inclined to believe the former because it would explain the very large estimate of the spillover that appears in Table 11.

The spillover from the Army's UVK program into Navy enlistments

[3]The difference for the Marine Corps was also small.

that appeared in the FY80-FY81 comparison was unexpectedly large. To explain it, we might have been led to suppose that some unaccounted for factor raised enlistments in all the services during FY81, explaining both the unexpected Navy spillover and the apparent response of Army enlistments to the program. However, extending the analysis to include FY82 data has made such an explanation unnecessary. Combining the two Navy estimates, we can place the spillover at approximately 5 percent, more in line with our expectations than the earlier 10 percent. The previous estimate of the direct effect of the UVK program on Army enlistments remains unchanged.

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